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ELEMENTS OF GALVANISM,

IN THEORY AND PRACTICE;

WITH A

COMPREHENSIVE VIEW OF ITS HISTORY,

FROM THE

FIRST EXPERIMENTS OF GALVANI

TO THE PRESENT TIME.

CONTAINING ALSO,

PRACTICAL DIRECTIONS FOR

CONSTRUCTING THE GALVANIC APPARATUS,

AND PLAIN SYSTEMATIC INSTRUCTIONS FOR

PERFORMING ALL THE VARIOUS EXPERIMENTS.

ILLUSTRATED WITH A GREAT NUMBER OF COPPER-PLATES.

BY C. H. WILKINSON,

Lecturer on Galvanism in Soho-square, Member of the Royal College of Surgeons, of the Philosophical Society of Manchester, and Associate of the Institute of Medicine of Paris, Lecturer on Experimental Philosophy to the City Institution, Honorary Member of the Physical Societies of Guy's, Bartholomew's, the Lyceum Medicum Londinense, and of the London Philosophical and Mathematical Societies.

> "ORATIONI ET CARMINI EST PARVA GRATIA, NISI ELOQUENTIA SIT SUMMA: HISTORIA QUOQUE MODO SCRIPTA DELECTAT."
>
> PLINY, JUN.

IN TWO VOLUMES.

VOL. II.

LONDON:

PRINTED FOR JOHN MURRAY, 32, FLEET-STREET.

1804.

95400

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Printed by B. M'Millan,
Bow-Street, Covent-Garden.

CONTENTS.

PART III.

CHAP. XIV.

Succinct account of the Voltaic pile, page 1—*Various curious and interesting experiments made with the pile,* 2—*Letter from Professor* VOLTA *to* M. DE LA METHERIE, *in which an explanation is given of the production of the galvanic phenomena,* 9—*The theory of* VOLTA *combated by the ingenious* Mr. NICHOLSON, *who brings forward several experiments to prove the powerful agency of fluids, in the production of the above phenomena,* 21—*Improved theory and doctrine of* VOLTA, 24—*Report made to the French National Institute on the subject of his new experiments,* 38.

CHAP. XV.

Interesting inquiries on galvanism, made by British philosophers since the discovery of the Voltaic pile, page 46.

CHAP. XVI.

Review of the researches of the different Continental Philosophers, after the discovery of the Voltaic pile, page 106.

CHAP. XVII.

General introductory observations, page 206.

CHAP. XVIII.

The principle of galvanism considered as an important agent of Nature, page 209—*Observations on the systems of* MESMER *and* PERKINS, 211—*What is meant by conducting and non-conducting substances, in electrical and galvanic experiments,* 214.

CHAP. XIX.

Different capacities of substances to contain the electric fluid, page 222—*Caloric one of the component parts of all bodies,* 224—*Change induced in particular substances by attrition,* 228—*Influence of the weather and seasons on the result of electrical experiments,* 230—*Effect of mercurial amalgams explained,* 231.

CHAP. XX.

The effects of electricity on air, page 233—*Principles of electroscopes and electrometers explained,* 236.

CHAP. XXI.

The influence of points on electricity explained, page 246—*Different states of electricity exhibited by an insulated conductor,* 249—*The electrophorus explained,* 253.

CHAP. XXII.

Theory of the Leyden phial, page 259—*Of spontaneous discharges,* 265—*Uses and advantages of the metallic coatings explained,* 269—*Theory of the different electric phenomena,* 273.

CHAP. XXIII.

The principle of galvanism explained; with a review of the different theories which have suggested themselves to physiologists, page 290—*Discovery of the decomposition of water by the galvanic process,* 294—*Chemical discoveries to which it may give rise,* 295.

CHAP. XXIV.

On simple galvanic combinations, page 300—*The arrangement of metals which forms the most perfect combination—Galvanism and electricity identically the same principle—The different modes in which they are produced—How far the development of electricity is a chemiscal process,* 302—*The influence of dissimilar metals on the muscles of a frog,* 303—*Any two different metals may form a galvanic combination,* 305—*Table of conductors and non-conductors,* 307.

CHAP. XXV.

The influence of two dissimilar metals on the muscular fibres of a frog, page 308—*How the electricity of the metals is disturbed,* 309—*The oxydation cannot go on till there is a conducting medium from one metal to the other,* 310—*The exquisite sensibility of the fibres of a frog to electricity, compared to the gold-leaf electroscope,* 313.

CHAP. XXVI.

On the influence of a single metal and two dissimilar fluids, on the muscular fibres of a frog, page

page 320—*Observations of Professor* ROBISON *on galvanic combinations*, 322—*The discoloration of a silver spoon from the eating of eggs, explained*, 323—*The combination of a single metal and two dissimilar fluids*, 324—*The discovery of* VOLTA *announced in a letter to Professor* GREN, 325.

CHAP. XXVII.

On the influence of charcoal—Mr. DAVY'S *galvanic combination with charcoal—Experiments made on that substance, page* 326—*In the order of the conductors, it stands next to metals*, 327—*Why it varies in its properties—Opinions of* PFAFF, HUMBOLDT, *and* LAGHI, 328—*Remarks of* FOWLER *and* WELLS, 329—ABILGOARD'S *proportion of carbon in venous blood—Observations on the charring of wood*, 330.

CHAP. XXVIII.

On the influence of animal substances, in constituting galvanic combinations, page 333—*The experiments of* ALDINI *explained on the foregoing principles*, 334—*Whether galvanism is connected with the nervous system—Observations of naturalists on the nerves of insects*, 338—*On fungi*

fungi as conductors, 339—*How a ligature on the nerves influences galvanism,* 340—*Remarks on the reproduction of nerves, when divided,* 341—*Conjectures on the infinitude of our sensations,* 342.

CHAP. XXIX.

On the galvanometer—Mr. PEPYS' *moveable coatings, page* 343—*Mr.* READ'S *condenser, adapted to the electroscope, improved by* CUTHBERTSON, 344—*The principle of action the same as in the Leyden phial—The state of electricity induced by a zinc, or by a copper-plate,* 346—*The part of the cap the best adapted to the application of the metallic plates,* 347—*Conjectures upon the cause of this difference,* 348.

CHAP. XXX.

On the Voltaic accumulation, page 350—*The* couronne de tasses, 352—*The pile*—CRUICKSHANK'S *trough,* 353—*The superior advantages of this construction—Method of preparing galvanic batteries—What liquids are the most convenient,* 355—*Order to be observed in connecting two or more batteries,* 357.

CHAP. XXXI.

How far the powers of galvanism are increased by the extent of metallic surface—The experiments of Fourcroy, Vauquelin *and* Thenard, *page* 359—*The shock is in proportion to the series, and not to the surface—Its powers in the deflagration of metals depend on the surfaces,* 360—*Conjectures as to the cause,* 361—*How far the surface is concerned, illustrated by experiments with six hundred small plates—The principles of the battery explained by the doctrine laid down in the chapter on electricity,* 365—*The necessity of the cells being insulated,* 371—*In a series of batteries, they should all be of equal powers and surfaces,* 372.

CHAP. XXXII.

On the decomposition of water, by Messrs. Carlisle *and* Nicholson, *page* 374—Richter's *experiments to shew that the effects are contrary to the theory of* Lavoisier, 377—*Remarks on the inaccuracy of the above experiments,* 379—*Dr.* Wollaston *and Dr.* Pearson *on the decomposition of water by electricity,* 380—*On the decomposition of inflammable fluids,* 383—*Remarks on the decomposition of water,* 384—*Dr.* Gibbes' *new theory,* 385—*Conjectures on this operation,* 386.

CHAP. XXXIII.

The comparative influences of electricity and galvanism on metallic substances—The metallic oxydes produced by electricity, page 390—*The deflagration of wires and laminated metals, on their oxydation,* 392—*Effects of deflagration on the gases*—Brugnatelli *on electrats,* 394—Van Marum *on the effects of combustion on quicksilver,* 395.

CHAP. XXXIV.

On the galvanic process performed by particular animals—The sentiments of the ancient naturalists on the torpedo, page 396—*The discovery of the gymnotus*—Walsh's *experiments on the torpedo,* 397—Hunter's *dissections*—Mr. Nicholson's *torpedinal theory,* 398—*The Voltaic principle applied to its explanation—Why torpedoes do not give out any light,* 399—Muschenbroek's *observations on the gymnotus,* 400.

CHAP. XXXV.

The function of respiration considered as a galvanic operation, page 401—*The lungs are, in structure, similar to the torpedinal system,* 402—*Observations on Dr.* Crawford's *theory of respiration,* 403—*His experiments on the capa-*

cities of bodies for heat, not correct, 407—*New experiments on steam*, 412—*Observations on heat*, 414—*Dr.* CRAWFORD's *estimates of the capacities for heat of arterial and venous blood*, 416—*On the theory of the French Philosophers as to respiration*, 419—*Calculations on the quantity of oxygen and nitrogen inspired*, 421—*Cases in which there is no circulation through the lungs in torpid animals—Remarks on these particular lungs*, 428—*Dr.* WELLS' *remarks on the change of colour in the blood—How far electricity is concerned—This principle changes the colour of the blood*, 430—*Remarks on the circulation*, 431—*It is not general, as supposed*, 432—*The colouring part of arterial blood never returns by the veins*, 436.

CHAP. XXXVI.

The application of galvanism to medicine, page 441—*Its effects in paralytic affections*, 444—*In cases of deafness*, 449—*The cases in which it is most beneficial*, 451—*Whether it is useful in amaurosis, or in gutta serena*, 455—*Observations in spasmodic affections—How far rigidity, and want of motion may be aided by this principle*, 456—*Its good effects in indolent tumors*, 457—*Cases of mental derangement considered*, 459—*Its use in cases of suspended animation, whether from drowning or hanging*, 463.

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Mr. WILKINSON, Author of this Work, informs the Public, that the different Galvanic Apparatus and Instruments described in this Work, either for philosophical or medical purposes, may be had at his house in Soho-square, on moderate terms.

N. B. A Battery consisting of fifty three-inch plates, is of a sufficient power for every medical purpose; and one of this size, with the requisite Apparatus, he sells for Five Guineas.

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THE
HISTORY OF GALVANISM.

PART III.

CHAP. XIV.

Succinct account of the Voltaic pile—Various curious and interesting experiments made with the pile—Letter from Professor Volta *to* M. de la Metherie, *in which an explanation is given of the production of the galvanic phenomena—The theory of* Volta *combated by the ingenious* Mr. Nicholson, *who brings forward several experiments to prove the powerful agency of fluids, in the production of the above phenomena—Improved theory and doctrine of* Volta *—Report made to the French National Institute on the subject of his new experiments.*

AT the commencement of the year 1800, Professor Volta made the discovery of the apparatus which has been denominated, after him, the Voltaic pile. A new epoch was thus formed in the history of galvanism; and it will be seen that this discovery gave rise to several other important ones, which are principally due to our learned countrymen. Among those who have pre-eminently distinguished themselves by their galvanic researches, since the above epoch, we have to cite Messrs. CRUICKSHANK, NICHOLSON, HENRY, DAVY, and PEPYS. The inquiries of Colonel HALDANE, and of several other British philosophers, are also highly interesting, and will be noticed in their proper places.

The Philosophical Transactions for the above year contain a letter from Professor VOLTA, in which he gives an account of his pile, and of a variety of curious experiments made with it. As it is the intention of the author, after having collected the different historical details relative to galvanism, drawn from the best sources and authorities, to devote the fourth and last part of this work to his own theory and opinions, and to enter into a full exposition of the apparatus employed for the various purposes of practical galvanism, it will suffice here to give a succinct, but clear idea of the Voltaic pile, as it was originally constructed.

Take any given number of disks, or plates of copper or silver, and an equal number of disks of tin, or, which is still better, of zinc, of the same dimension. Provide an equal number of round pieces of pasteboard, leather, cloth, or of any substance whatever capable of retaining the moisture it has imbibed for a considerable time. Silver or copper coins may likewise be employed for this purpose. Now, form a pile, by laying alternately the zinc over the silver, the pasteboard over the zinc, and thus consecutively. If it be intended that the pile should have a certain degree of elevation, it ought to be supported between three glass tubes. When it is thus constructed, the apparatus is in a state to perform the functions required of it. A pile of this description, so long as it continues in a well humected state, appears to be a continued and inexhaustible source of a current of electricity, which permeates every conductor brought in contact with the two extremities of the apparatus. If this conductor be an animal, and if the two parts of its body, by which the top and bottom of the pile are touched, be moistened, a condition which is essential to the effect, the animal will receive, on each of the contacts, repeated indefinitely, a true electrical shock, more or less violent, according to the circumstances. The shock will likewise be felt if a part only of the pile be comprehended in the electrical circuit; but the sensation will then be much weaker. It would appear that its intensity is augmented in a ratio much greater than that of the portions of the pile, comprised between the two points of contact.

tact. It has also been noticed that this sensation is increased, nearly in proportion to the squares of the heights, intercepted between the above points.

When experiments are made with the Voltaic pile, the sensation which is felt resembles the effect of a weak charge from a very large electrical battery. Its action is so inconsiderable, that its influence cannot penetrate through the dry skin. It therefore becomes necessary to moisten a part of each hand, and then, with a piece of metal held in each, to touch the bottom and top of the pile, or the conductors which correspond with its two extremities. The extremities of the conductors may likewise be made to lead into two separate vessels filled with water, into which a finger of each hand should be plunged. The shock becomes more powerful, in proportion to the number of the pieces brought in contact. Twenty give a shock, which, when the due precautions are taken, is felt in the arms. With a hundred pieces it is felt in the shoulders. The current of electricity acts on the animal system, so long as it continues to make a part of the circuit. If any one who subjects himself to the experiment has the smallest cut, burn, or excoriation, near the extremities in contact with the pile, he feels a painful, and, indeed almost insupportable, sensation at the part affected.

By the help of his condenser, VOLTA proved that these effects are purely electric. He even ascertained the kind of electricity, and produced the spark. It ought, however, to be observed, that it was likewise obtained, without having recourse to the condenser, at the extremity of a pile of fifty-seven pieces of silver, and as many of zinc. VOLTA remarked that its action, in the parts where the cutis had been laid bare, was sharpest on the negative side of the apparatus, that is, when the electrical current flowed out from the wound, than when it entered. This fact had been previously noticed in the case of the common spark.

The theory brought forward by this celebrated naturalist is as follows: He observes that all the conductors, or *anelectrics*, the conducting faculties of which are comparatively different, have the property of occasioning an electric current, when they are brought in contact. In this case it suffices that metals touch each other at a single point; but a greater extension ought to be given to humid substances. The results of a great number of experiments made by VOLTA, convinced him that the effects are the same, whether the zinc and silver be brought in contact, or a communication established by several different metals, provided the water be made to communicate with the zinc and silver only. When the latter of these metals is employed, the salt water is preferable to the alkaline ley, which, again, is the most desirable fluid, when tin is made use of. The intensity of the effects becomes greater, in proportion as the temperature of the apparatus is raised.

VOLTA was surprized to find that the galvanic flash of light* was not stronger with this apparatus than with a single pair of plates. It is true, that the flash was produced, when the conductor of the battery was applied to any given part of the face, and even to the breast. The strongest action ensued, when the metal by which the battery was terminated, was held between the teeth, and made to rest on the tongue. A convulsive shock was then felt in the above part, as well as in the lips, the flash appearing to the eyes, and the savour being perceptible in the mouth. After having introduced into each ear a metallic blunted probe, VOLTA directed the passage of the electric shock through his head, and, suffering the communication to be kept up in the battery, heard a peculiar sound, similar to that of ebullition or decrepitation. This experiment he did not think it safe to repeat. He could not, by any of the trials he made, affect the sense of smelling; and this he ascribed to the impossibility of circulating in the air this description of electricity.

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* Thus is termed the sensation which is perceived, whether the eyes be open or closed, at the instant of the contact of two different metallic surfaces, of zinc and silver more particularly, which are made to communicate respectively with the gums of either jaw. This flash of light, produced by a pile which contained an hundred and eleven plates of silver, was so powerful, that the physiologist who made the experiment was not disposed to repeat it.

As the apparatus loses its energy, in proportion as the metallic and other substances laid on each other become dry, our naturalist endeavoured to prevent this effect, by enclosing the pile either in wax or pitch. He contrived to enclose in this way two columns, each composed of twenty plates, which acted perfectly well during several weeks, and which, he had reason to expect, would continue to do so for the space of several months. The particular combination which he considers as the most useful and instructive, is the one formed by a range of glasses, or cups, not of a metallic substance, containing either warm water or brine. Into each of these a plate of silver, and another of zinc, are plunged, but without reciprocally touching each other. Between these glasses, or cups, metallic communications are established, and disposed in such a way as that, provided they reach, on the one hand, the zinc contained in one of the glasses, they should reach the silver in the other; and that from the zinc in the latter they should extend to the silver in the one by which it is followed; and thus consecutively throughout the whole of the assemblage. When any one places himself in the range of the battery, between the first and the last of the glasses, he feels the commotion. It is essential that the plates of metal, plunged in the fluid, should have, at the least, a square inch of surface. With respect to the communications from one of the glasses, or cups, to another, they may have as small an extent as the operator may wish.

Volta, after having adopted, in a certain degree, the conjectures of our very intelligent countryman, Mr. Nicholson, on the cause of the effects produced by the torpedo *, considers his new apparatus as having a very striking analogy with the electrical organ of that fish. Mr. Nicholson follows up the observation that, in consequence of Volta's discovery, there no longer remains any doubt but that galvanism ought to be comprehended among the electric phenomena, by expressing his surprize that the learned Italian professor has not directed any of his inquiries to the chemical phenomena of galvanism announced by Fabroni, and more particularly

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* Nicholson's Journal, vol. i. 4to. p. 358.

ticularly to the rapid oxydation of the zinc, by which the experiments are invariably accompanied.

The following letter, on the phenomena of galvanism, was addressed by Professor VOLTA to M. DE LA METHERIE. The latter had demanded of him a concise account of his experiments, by which he attempted to demonstrate what he had always maintained, namely, that the pretended agent, or *galvanic fluid*, is merely the *common electric fluid*, incited and put in motion by the simple *mutual contact of different conductors*, and of metallic ones more especially. By these experiments the learned professor flattered himself he had proved, that two metals of a different nature, when combined, produce, in a certain degree, a true electricity, the force and quality of which may be ascertained. That the powerful, and, indeed, miraculous effects of the new apparatus, the parts of which may be denominated *electro-motors*, whether in the form of a pile, or of a range of glasses, or cups, are nothing more than the aggregate sum of the effects of a series of several similar couples of metallic plates. And, lastly, that the chemical phenomena, which are obtained by the decomposition of the water and other fluids, by the oxydation of the metals, &c. are the secondary effects of the above electricity, of the continual current of electric fluid, which, by virtue of the aforesaid action of the connected metals, is established as soon as a communication is formed, by a conducting arc, between the two extremities of the apparatus. This communication, when once made, is maintained, and continues until the circuit is interrupted.

The truth of these observations was proved by several of the experiments made by M. Volta, with his small portable apparatus, in the presence of M. Pictet, the naturalist of Geneva, and of several other persons. These experiments were afterwards repeated, and form the basis of a very extensive memoir, which our naturalist read at the French National Institute, and the substance of which will be given hereafter.

"I began (observes Volta) by shewing, by several nice, yet simple experiments, that unequivocal electrical phenomena are to be obtained by the simple contact of two different metals, without the intervention of any humid substance. These experiments ought therefore to be considered as fundamental. To render this electricity, which is so feeble, that, without having recourse to other artifices, it would be imperceptible, both sensible and manifest, I employ my electrometers made of fine straws, combined with my condensers, the best of which are made of two metallic disks, having their smooth and polished surfaces nicely brought together, and covered with a slight layer of sealing wax, or, in preference, with a good varnish of gum lac.

" The first mode of making this experiment, was to take two disks, or plates, one of copper, and the other of zinc, and to hold each of them by an insulating handle, made of glass coated with sealing wax. In this way their polished surfaces were applied to each other for an instant, and, having been dexterously separated, they were brought in contact with the electrometer, which then marked, by a divergence of about a line of its straws, the electricity each of the disks had acquired. That the one which was of zinc had received the positive electricity, (el. +), and the one of copper, the negative, (el. —), was readily to be ascertained, by bringing near to the electrometer a stick of sealing wax which had been previously rubbed.

" It ought here to be noticed, that, in this experiment, the two plates, at the same time that they are, in consequence of their mutual contact, as two different metals, *motors* of electricity, perform also the office of condensers, when their extensive surfaces are brought together. This is the reason why their contrary states of electricity are so nicely counterbalanced;

and it likewise explains why the positive electricity in the plate of zinc, and the negative in the plate of copper, which would not otherwise rise higher than about the sixteenth part of a degree, a state, indeed, in which these electricities continue so long as the plates are applied to each other, rise, as soon as they are detached, to one, one and a half, or two degrees, and even more.

"Such a state of electricity, it must be granted, is but trifling; and does not satisfy those who wish to see the effects on a large scale. In order, therefore, to produce still more striking electric phenomena, I commonly make use of a second condenser, placed on the electrometer itself, and proceed in the following manner. I apply to each other the plates of copper and zinc, and, having separated them repeatedly, I bring one of these insulated plates, as often as the separation is effected, in contact with the upper disk of the condenser, and the other, insulated in the same way, in contact with the lower disk, which rests on the electrometer. Having repeated these contacts ten, twelve, or twenty times, on elevating the upper disk of the above-mentioned condenser, the electrometer which supports the lower disk, rises to ten, twelve, fifteen, twenty degrees, &c.

"It might be apprehended that, independently of the action of the condenser, the extent of the contact, between the two diverse metals, contributes greatly, as such, to the elevation of the electricity to the above-mentioned degrees, and that it would be much lower, if they were merely to touch each other at some particular points. I demonstrate, however, the contrary, that is, that, in either of these cases, the electrical tension reaches, during the contact, the same point, which is nearly the sixtieth part of a degree of my electrometer made with fine straws, when the two metals are zinc and copper. When the plates are composed of zinc and silver, it is somewhat more considerable. With respect to this tension, as it requires a much larger quantity of the electric fluid, in the plate which performs the office of condenser, before it can be made to condense sixty, a hundred, a hundred and fifty, or two hundred times, it is on this account that one, one and a half, two degrees, &c. are obtained.

" To prove that a contact of two metals, of a small extent, and even at a few points only, displaces the electric fluid in such a way, as to carry, in these metals, the tension to the same degree, I join a small plate of copper with another of zinc, either similar or dissimilar, with respect to its shape and size, applying them to each other at a few points only, or at a still greater number of points, or even soldering them end to end."

Here follow several figures, by which this theory is elucidated, but which it is unnecessary to give in this place, as the subject will be more particularly inquired into hereafter. Professor VOLTA now proceeds as follows: "The action by which the electric fluid is excited and put in motion, is not produced, as has been falsely supposed, on the contact of the humid substance with the metal, or, granting this to be the case in a certain degree, it is unworthy of consideration, when compared with the action which ensues, as my experiments have fully proved, on the contact being established between different metals. Consequently, the true element of my electromotive apparatus, in the form of a pile, or of a range of cups, or constructed in any other manner, conformably to the same principles, consists in the simple metallic pair of plates, composed of two different metals, and not, as a great number of naturalists have supposed, in a humid substance applied to a metallic substance, or comprised between two different metals. The humid layers, in a compound apparatus of this description, are therefore merely introduced to establish a reciprocal communication between all the metallic pairs of plates, so arranged as to impel the electric fluid in one particular direction; or, in other words, to make them communicate in such a way, as that no part of the action should take a contrary direction.

"After having nicely ascertained the degree of electricity I obtain from a single pair of metallic plates, by the help of the condenser which I employ, I proceed to shew that with two, three, four, or a greater number of pairs, well arranged, that is, all of them turned in the same direction, and communicating with each other by as many humid layers, which, as I have demonstrated, are necessary to prevent any diverse actions, I obtain a double, a triple, a quadruple degree, &c. Consequently, if, by the means of a single pair, the condenser be electrified in such a ratio as to give to the electrometer three degrees, for instance; with two pairs, six will be obtained; with three pairs, nine; with four, twelve, &c. if not precisely, pretty nearly at the least.

"Thus is a small pile constructed, which does not, however, enable the electrometer to bestow any indications without the aid of the condenser. To the end that it may manifest them without delay, and reach a full degree of *electrical tension*, which, as it is marked by half a line only of divergence of the points of the straws, is scarcely perceptible, it is necessary that such a pile should be composed of about sixty pairs of plates of copper and zinc, or, which is still better, of zinc and silver, according to the proportion of the sixtieth part of a degree bestowed by each pair, as I have had occasion to remark. Under these circumstances the pile likewise gives shocks, if its two extremities be touched with the moistened fingers; and still more powerful ones, if they be touched by metals, the extensive surfaces of which are grasped by the hands, in a humid state. By this last method a much better communication is established.

" In this way an apparatus, whether in the form of a pile, or of a range of cups, composed of thirty, and even of twenty pairs of plates, may be made to give shocks, provided the metals are well cleansed, and, more particularly, if the humid layers which are interposed, instead of being moistened with water, are wetted with a pretty strong solution of saline substances. This is not, however, because these saline fluids augment, in a positive manner, the force of the electricity; but because they facilitate the passage of the electric fluid, the current of which meets with fewer obstacles. That they are better conductors than pure water, has been demonstrated by several decisive experiments.

" To establish this fact, and to make it evident to those who could with difficulty be persuaded that the electric force is, if not precisely, pretty nearly the same, whether the humid layers have been soaked in pure water or in salt water, notwithstanding there is so great a difference in the shock which is felt, I have frequently made the following experiment: I took thirty cups, or drinking-glasses, and constructed with them the particular apparatus which I denominate *couronne de tasses*, by putting into each of them a sufficient quantity of pure water, and establishing a communication between the first and second, the second and third, and thus on, consecutively, until the last, by the means of metallic arcs, which were terminated on the one side by a thin plate of copper, and on the other by a plate of zinc, the whole of them being turned in the same direction. The apparatus being thus arranged, I tried its electrical force, by establishing a communication between the first of the cups, or glasses, and the floor; and having applied the condenser to a piece of metal partly immersed in the cup in question, it gave me, as soon as I had withdrawn it, and had separated one of the disks from the other, in a proper manner, and without any delay, forty, sixty, or a still greater number of degrees, according to the condensing force. I likewise tried the shock in the most advantageous manner, and found it to be very weak. After having been fully satisfied, both of the degree of electricity, and the weakness of the

the shock, I threw a pinch of salt into each of the cups; and, on a repetition of the trials, I found that the electricity was not augmented, the condenser still affording me no more than the forty or sixty degrees, as before, although the shocks were incomparably stronger.

"I have already informed you, that, by means of the apparatus, such as it has been described, I charge a Leyden phial, whatever may be its capacity, and even a large battery. I charge them in an instant, or, to speak more correctly, in less than the twentieth part of a second, and pretty nearly to the same degree as the apparatus itself; namely, to about a degree of tension, provided the apparatus is composed of sixty pairs of plates; to two degrees, if it contains a hundred and twenty, &c. In such a case I can, by the help of my condenser, draw a pretty strong spark from small jars charged in this manner, a great number of similar sparks from large jars, and an almost unlimited number from the apparatus itself.

"I have informed you, that large jars, when thus charged, gave me moderate shocks, and the batteries, pretty strong ones, which extended to above the joint of the elbow. Those produced by a battery provided with a coating of ten square feet, and charged, in less than the twentieth part of a second, by an apparatus having two hundred pairs of metallic plates, are so strong as to be nearly insupportable. I have not as yet made any trials with larger batteries; but there is every probability that the force of the shocks would be augmented in proportion to their size, until a certain limit, on the extent of which I cannot decide. It would thus be possible, with batteries of forty, sixty, and a hundred feet square, to give pretty strong shocks, if they were to be charged by the sudden and transitory contact of a pile of sixty pairs only, and even of forty or thirty, or perhaps still less.

"I have explained to you the mode which ought to be followed, to give a successful issue to these experiments. It is more particularly necessary to avoid, with extreme care, the smallest interruptions in the communications between the conductors and the coatings of the jars, as well as between each other. This is still more essential, when the electromotive apparatus, composed of a few pairs of plates, is not very powerful; seeing that it is unable to overcome the slightest obstacle it may meet with in its passage, or which may resist the course of the electric fluid.

"Finally, I have pointed out to your notice, that these experiments confirm, in the most decisive manner, what had been already suggested by the preceding ones; namely, that the quantity of electric fluid put in motion by my apparatus, is much larger, within any given space of time, than the quantity which can be obtained by the common electric machines. A more abundant supply is procured by the means of my apparatus than by the latter, when the object is not an accumulation of the electric fluid in insulated bodies, with a view to raise the electricity they contain to a high degree of tension, which may be effected by the above-mentioned machines, but not by the pile, and other similar contrivances, unless a condenser be employed; but when the aim is to establish a constant current of the fluid in question, to be kept up by an unceasing action in a circuit of non-insulated conductors. In this latter case, an apparatus of sixty pairs of metallic plates, and even of thirty, pours out every instant, or, in other words, within any given time, a greater quantity of the electric fluid, provided it does not meet with any obstacle, and is not obstructed by the small and disproportionate capacity of the recipient in which it is collected, than the best and most powerful electrical machine, constructed either with a cylinder, or with a plate of glass. In reality, where is an electrical machine to be found, which is capable of charging to a degree, or even to half a degree, a very large battery, in less than the eighth part of a second, and which is enabled to infuse into it such a quantity of the electric fluid as may suffice, when it is afterwards drawn out by the help of a condenser, to produce a great number of sparks, following each other in succession? This is, notwithstanding, the case with one of my machines.

"The other experiments, a part of which were witnessed by you, regard the different electroscopic phenomena produced by the apparatus, according as either of its extremities is made to communicate with the ground, or both of them, or neither, or as they simply communicate with each other and with the ground at the same time; and finally, according as the communications are made by perfect conductors, by more or less imperfect ones, &c. Each of the above circumstances modifies in a singular way, and gives a great variety to the result, which is frequently as curious as it is irregular. This I think I can explain in a satisfactory manner, without deviating from my own principles, and from the best electrical theories, by a nice attention to the mode in which bad or imperfect conductors act. These details would lead me too great a length."

The following observations on the above letter, are from the pen of the intelligent Mr. NICHOLSON*.

" Signior VOLTA, and many of our philosophical neighbours in France, will no doubt think, on reconsidering the facts, that they have been too precipitate, in admitting the electric energy as the only effective agent in the phenomena of the pile, and that fluids act merely as conductors. The recent interruption of correspondence may probably have rendered the contents of my Journal little known in France, otherwise the learned author of the above letter would have found a fatal objection to that part of his theory, which gives all to the metals, and nothing to the fluids, in Mr. DAVY's galvanic pile*, consisting of one metal throughout, but with different stata of fluids; for example, thus: metal, cloth soaked in dilute nitrous acid, cloth soaked in water, cloth soaked in sulphuret of potash; then the same metal and nitrous acid, water, and sulphuret; the metal, &c. Or, if a trough be used, the separation between the acid and sulphuret may be made by a plate of horn, and the two fluids may be connected by a slip of wetted paper hung over the edge of the horn, which will not cause the fluids to mix, because water is lighter than either. The metals separately and successfully tried were, silver, copper, zinc, and lead.

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* Phil. Journal, vol. i. 8vo. p. 142.

" To this I will here add, from conversation, an experiment of the same philosopher, which is no less conclusive as to the direct effect of the fluid in this apparatus; because the same electric power is made to move either from the top or bottom of a pile of two metals, according to the nature of the interposed fluid. If a pile of iron and copper be constructed as usual, with water interposed, the iron becomes electrified *plus*, and the copper *minus*; but if the same, or a similar pile, be constructed with no other difference, than that sulphuret of potash is used instead of water, the iron is electrified *minus*, and the copper *plus*. In the first case the iron is oxyded; but in the second there is no oxydation of this metal, and the copper is oxyded, and probably sulphurated.

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* An account of this contrivance will be given in the sequel.

"Lastly, we have another instance of the power and importance of fluids in the article* which follows the present, where charcoal alone is used, and the leading condition is, that two different fluids shall be used.

"As we know by the experiments of M. De Saussure, and many others, that chemical changes do disturb the equilibrium of electricity, and they certainly take place in the pile, it seems at least probable, that these may have the chief agency in the apparatus. With regard to the principle of the electric-motors of Signior Volta, I must observe, that Bennet made many direct experiments by the application of different metals, by the single and double touch, to the plates of the doubler, followed by the production of electricity, by him called adhesive electricity, which were published in his new experiments on electricity. Others were made by CAVALLO, on the electricity produced by the contact or stroke of a piece of metal let fall from the hand, for the most part, on an insulated metallic plate, which were published in the third volume of his Electricity, in 1795. I do not know the date of VOLTA's experiments, but believe them to be much later than those of the same kind by BENNET. This last philosopher, as well as CAVALLO, appears to think that different bodies have different attractions or capacities for electricity; but the singular hypothesis of electro-motion, or a perpetual current of electricity being produced, by the contact of two different metals, is, I apprehend, peculiar to VOLTA."

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* An analysis of this article will be introduced in its proper place.

Towards the close of the year 1801, Professor VOLTA, accompanied by his friend and colleague BRUGNATELLI, paid a visit to Paris, to hold a conference with the *savans* of that capital, on various scientific objects, and principally on the phenomena of the galvanic pile; a discovery which does him so much honour, and which interests, in so high a degree, the progress of the physico-chemical sciences. In the sitting of the class of physical and mathematical sciences of the French National Institute,

stitute, held on the 7th of November 1801, this celebrated naturalist read a part of a memoir, written by him, and containing the detail and results of his new experiments on galvanism. It should be observed, that the aim of these experiments was to bring fresh and striking evidences of the identity of the galvanic and electric fluids, agreeably to the hypothesis he had invariably maintained. The reading of the memoir was terminated at the two sittings next following; and on the 2d of the ensuing month, December, a gold medal was unanimously decreed to the learned professor of Pavia.

The following extract from the above report, which has made so strong an impression on the scientific world, will, we flatter ourselves, be highly gratifying to our readers. It will be found to contain a summary, but clear, exposition of the new and improved theory and doctrine of VOLTA.

Our naturalist, after having observed that he had already explained, in his letter to M. DE LA METHERIE, the motives by which he had been led to advance that the galvanic fluid, or agent, is no other than the true electric fluid, proceeds to the observation, that the denomination which is employed, whether it be that of galvanic electricity, or that of the galvanic agent or fluid, is of little importance, provided there be not any discordance as to the facts. To attain this aim, and to put an end to every idle contestation, arising from the want of a due and mature consideration of the subject, he has deemed it necessary, by a recapitulation of several principles, to which little or no attention had been paid, to obviate the difficulties and objections which had not escaped him, although he had passed them over in silence. "Accordingly (he observes) the present memoir may be considered as a sequel of the letter I have alluded to above, or, rather, as forming a part of several other memoirs which I purpose to publish hereafter, notwithstanding this one contains in itself a complete system both of theory and doctrine."

The memoir is divided into twenty-nine sections. In the first of them VOLTA explains the three most forcible objections which have been brought forward, against the homogeneity of the galvanic and electric fluids. They are to the following effect:

1st, The absence of several electrical phenomena, and the scarcely perceptible development of several others, when compared with the shocks, painful sensations, &c. produced by the simple contact of two metals of a different nature, such as silver and zinc, as well as by the assemblage of several pairs of these diverse substances,

stances, communicating with each other by the means of a humid conductor.

2dly, The imperfect transmission of the galvanic fluid, or principle, whether it is produced by the simple galvanic apparatus, which has been long known, or derives its source from the compound apparatus, invented by the author. To this consideration may be added the property possessed by certain substances, such as rarefied air, flame, &c. of suspending, and even annihilating, the action of the apparatus, notwithstanding these substances are considered as excellent conductors of electricity.

3dly, The strange and surprizing decomposition of water, when it is subjected to the above apparatus. This result it would be difficult to ascribe to a very feeble electricity, which the most delicate electrometers render scarcely perceptible; seeing that it is not produced, either by the most powerful discharges of a common electrical machine, or by a very rapid and long continued electric current.

Such are the doubts and objections which VOLTA endeavours to remove in the present memoir. He considers those by whom they have been made, as being but little versed in the science of electricity, more especially in the part which embraces electrometry.

To reply in a satisfactory manner to these objections,

jections, he found it necessary to ascertain with precision the degrees of force which the electricity receives from the contact of two metals of a different nature; a contact by which they are rendered not merely conductors, but even exciters, or *motors*, of the electric fluid. For this purpose he made choice of the two particular metals the most opposite to each other, and at the same time the most active, namely, silver and zinc, the efficacy of which is augmented, instead of being diminished, when they contain a certain proportion of other metallic substances. When they are both of them well cleansed, and their surfaces polished, the instant they touch each other, in a direct way, at one or several points, they displace the electric fluid, and destroy its equilibrium, so as to make it pass from the silver to the zinc, and to be rarefied in the one, while it is condensed in the other. In this double state of rarefaction and condensation it is maintained, provided the two metals have not any communication with other conductors, which, agreeably to the laws of equilibrium, may supply to the first the dose of electric fluid it has lost, and deprive the second of the dose it has acquired.

After having described his electrometer made with fine straws, which indicates the sixtieth part of a degree of positive electricity in the former of these metals, and of negative electricity in the latter; and after having made a comparison between this electrometer and the one invented by BENNET, our author proceeds to observe, that he can render this electricity sensible to either of the above instruments, and ascertain either its positive or negative quality, by the help of his newly invented condenser, a description of which he gives, as well as an explanation of the uses to which it may be applied. He complains that it has not in general been employed with all the attention necessary to ensure the success of the experiments.

This conductor having been disposed in the manner which he points out, VOLTA proceeds to his experiments, the detail of which could not be comprehended without the plates by which they are accompanied. It will suffice to observe, that the experiments which he describes, prove that the force which gives the impulsion to the electric fluid, instead of arising from the communication of such or such a metal, with one or several humid conductors, is produced by the reciprocal contact of the two metals, at the precise part where they touch each other.

With respect to the suspicion which may be entertained, that the electric fluid is displaced by the contact either of the silver, or of the zinc, with the fingers by which it is held, or with any other humid conductor which may supply their place, he observes that this suspicion falls to the ground, if the experiments be repeated without the hand, or any other humid substance, touching the two laminas of silver and zinc; or, in other words, without there being any contact, unless between the two metals themselves. This may be effected in several different ways, which our author describes.

In his letters to Professor GREN*, VOLTA had already proved the affirmative of the question, whether the electric fluid receives a certain degree of impulsion from the direct contact of a metal with a humid conductor. He now observes, that this impulsion is so weak when water is employed, either pure or combined with salt, as not to admit of any degree of comparison with the one which results from the communication of different metals, such as zinc, silver, or copper. He excepts, however, several of the concentrated acids, as well as several alkaline liquors, and alkaline sulphurets, &c. which, by their contact with different metals, give a very sensible impulsion.

Having determined, with great precision, by several other experiments, the particulars of which he does not detail, the ratio of the impelling or accumulating force of the conductor he employed, he found it easy to discover the degree of the force or tension of the electricity, respectively acquired by each of the two plates of silver and zinc, when brought in contact. This tension was either maintained or renewed, by keeping up or renewing the communications. Its different degrees were calculated by those of the condensation readily obtained with a good condenser, the surfaces of which had been polished, and carefully coated with varnish.

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* See vol. i. of this work, p. 99.

He thus proved, by the aggregate of his experiments, which were varied in several different ways, that the sixtieth part nearly of a degree of his electrometer made with the fine straws, constitutes the electrical tension occasioned by the mutual contact of zinc and silver. In this tense state, the electricity is, as has already been observed, positive in the former of these metals, and negative in the latter. Several other metals, which differ less in the power of inciting the electric fluid, produce a smaller tension in proportion as their difference, in this respect, is diminished, and according as they are less distant from each other in the following scale: *silver*, *copper*, *iron*, *tin*, *lead* and *zinc*. In this scale the first of these metals drives the fluid into the second, the second into the third, and thus consecutively. VOLTA gives it a very extensive range, and makes it embrace several other metals, semi-metals, pyrites, charcoals, &c. He observes, that there are other substances which appear to drive the electric fluid into various metals, more particularly into zinc, with a greater force than silver. These substances are plumbago, several kinds of charcoal, and, more especially, crystalized black manganese, the communication of which with the zinc produces a tension nearly twice as great as that of the silver and zinc, that is, it is carried to the thirty-fifth part of a degree.

But, to the end that this effect may be produced, it is necessary that the plate of silver should communicate with good conductors, in which it may find the electric fluid it is to impart to the zinc, and which the latter is to deposit in the condenser, to the end that the electricity may be accumulated to the degree above pointed out. It follows from hence, that the electrometer cannot, even with the help of the best conductor, denote any portion of electricity in two different metals which touch each other, provided one of the two does not communicate with a sufficiently capacious recipient, at the same time that the other transmits the electricity it has successively acquired, to the condenser, in which it is accumulated.

"All these experiments," observes M. VOLTA, "which alike gave me two, three, or four degrees of positive electricity, in a plate of zinc, and of negative, in a plate of silver, are the result of the same principles, that is, that the silver drives the electric fluid into the zinc in such a ratio as to produce a tension of about the sixtieth part of a degree of positive electricity in the latter of these metals, and of negative electricity in the former. This tension is produced by a dose of the electric fluid, communicated by one of the plates to the other, which is the more considerable in proportion as these plates, by a due approximation, and by a reciprocal counterbalancing of each other, answer the purpose of excellent condensers. It is accordingly proved, that the positive *electrical tension* in the zinc, is, as well as the negative in the silver, about the sixtieth part of a degree; and that it is maintained in this state during the whole of the time that these two metals touch each other, without having any communication with other condensers, susceptible of receiving and transmitting the electric fluid, impelled and displaced by this tension."

VOLTA proceeds to adduce the most satisfactory and conclusive proof, that the degree of the above cited electrical tension, occasioned by the mutual contact of the metals which have been pointed out, is precisely what he has stated it to be. This proof he draws from a multitude of other experiments, the results of which he gives, made with several couples of metallic plates. After having pointed out the most simple mode of making these experiments successfully, he insists on the necessity of placing, between each of the pairs of metallic plates, a humid layer. This condition is so essential, that it is impossible to obtain an augmentation of electricity, that is, a tension greater than the sixtieth part of a degree, with simple pieces of zinc and silver brought together, whatever may be their shape and form, without the intervention of a third humid conductor, endued with a similar energy.

This electrical augmentation cannot even be effected by the aggregation of three, or of several metals, with the intervention of humid conductors, inasmuch as there exists a certain ratio between the metallic conductors of the first class, relatively to the force with which they impel the electric fluid, the one into the other. The force, or impulsion, given by two metals to the electric fluid, is, according to VOLTA, equal to the sum of the forces of those which are found in the series, or graduated scale, between these two metals, Thus, whether the intermediate metals do, or do not, enter into the apparatus constructed simply of metallic substances, or whether they are all of them interposed between the two by which the extremities are formed, or, lastly, whether a part of them only be thus interposed; whatever disposition of them may, in short, be made, there will not be any change in the electric force, which will be absolutely the same as when the first of the metals in the series is in immediate contact with the last.

VOLTA speaks of a discovery which still remains to be made, and which, however difficult it may appear, may, notwithstanding, in his opinion, be effected, namely, that of a new *electro-motor,* composed entirely of solid substances. "Would it not suffice for this purpose," he observes, "to find a solid conductor, either deprived of every force of impulsion, or possessing it in any other point of view except in the one I have cited, which should be made to supply the place of the humid conductors, between the common pairs of plates of diverse metals? The correspondence of action and of force, inherent in metals, has been ineffectually sought in the substances which hold an intermediate station between them and fluid bodies."

Our naturalist proceeds to inquire, whether the ratio which subsists in the electrical impulsive force of the conductors of the first class, and which does not extend from that class to the second, is to be again found among the conductors of the latter order, when reciprocally compared with each other. He observes that, even on the supposition of this ratio, it would be impossible to form, with these substances alone, in the same way as with simple metals, an instrument possessing a sufficient energy to produce the shocks and sparks. Nature has confined this inestimable advantage to the electrical organs of the torpedo, and of the Surinam eel, *gymnotus electricus*, which are entirely composed of humid conductors, without any metallic portions. *This artifice*, observes VOLTA, *we shall, according to every probability, one day contrive to imitate.*

We must consequently either suppose, in these bodies, a different ratio in their electrical actions, or admit, in the second class, a sub-division, a third class of conductors, which may accord reciprocally in the exercise of the impulsive power, at the same time that they are not in agreement with the humid conductors, or conductors of the second class.

VOLTA concludes by giving it as his opinion, that the composition of this third class of substances, which are at the same time conductors and *motors*, may be formed by substances drawn from a humour, which, as it coagulates and becomes fixed in a degree imperceptible to our senses, cannot with propriety be denominated a humid substance. It is presumable, according to him, that in the electric organs of the torpedo, the small layers, or pellicles, placed one above the other in each of the columns, are alternately formed of conductors, partly belonging to the second, and partly to the third class, arranged in such a way as that each layer, or heterogeneous pair of the third class, is separated by a conductor, that is, by a humid layer, belonging to the second class. "Such is," observes he, "the idea I have conceived of the electric organ of the torpedo, solely composed of conducting substances. This organ cannot otherwise be compared than to an electrical apparatus, the construction, form, and effects of which are nearly similar."

We have thus given the substance of Professor VOLTA's memoir, without omitting any of the essential points which tend to establish the new theory he has adopted. We are, nevertheless, of opinion, that, to comprehend it fully, it is necessary, as has already been observed, that our readers should peruse the memoir itself, and this for the following reasons: because it contains a chain of facts and arguments the whole of which could not have been introduced into any extract or analysis; and because its author, in propounding his ideas, frequently refers his reader from one section to another. The extract, however, which will be given hereafter of M. VAN MARUM's letter to Professor VOLTA, will throw a new light on the above theory, and render it more sensible to the perceptions. It is necessary, in the mean time, to say something of the interesting report made to the French National Institute on the memoir in question.

As soon as it had been read by its author, a commission* was appointed by the Institute; and the report was made on the 2d of December, 1801, by Biot, one of the members. The reporter sets out by observing, that Volta had been the first to announce, that the animal arc which he had introduced into his experiments, was almost exclusively calculated to receive and manifest the galvanic influence, but was very little, if at all, concerned in its production; and likewise, that the muscular irritation, which had been originally considered as the important part of the phenomenon, was, in his opinion, merely an effect of the electrical action, produced by the mutual contact of the metals of which the exciting arc is formed. This opinion, while it found several partisans, having been strongly controverted by other physiologists, gave rise to a great variety of experiments, which, however, had not afforded any decisive or positive result, when the former report * was made by M. Halle, to the class of mathematical and physical sciences.

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* The commission was composed of the following very respectable names: Laplace, Coulomb, Halle, Monge, Fourcroy, Vauquelin, Pelletan, Charles, Brisson, Sabatier, Guyton, and Biot,

The researches by which Volta, in pursuing the route he had traced out, endeavoured to attach to his early discovery all the phenomena which galvanism presents, were not known at that time. This learned naturalist has since communicated several others of an equal importance, which he has endeavoured to connect by a very ingenious theory. "If any thing," observes the reporter, "still remains to be done, to ascertain with precision the laws of this singular action, and to subject them to a rigorous calculation, the principal facts on which it is founded, appear, at the least, to have been invariably fixed."

The commission now proceeds to give an account of the fundamental experiments of Professor Volta; of the application he made of them in establishing his theory; and of the readiness with which he repeated them several times before the members of the commission, who were thus enabled to ascertain the truth and precision of the results.

The principal fact, from which all the others are derived, has been already touched upon in the extract we have given above from Volta's

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* See vol. i. p. 363, of this work.

memoir. It is, however, essential that it should be more particularly adverted to in this place. It is as follows: If two diverse metals, insulated, and possessing their natural quantity only of electricity, be brought in contact, it will be found that, as soon as they are withdrawn from this contact, they will possess different states of electricity, or, in other words, one of them will be in the positive, the other in the negative state. Thus, in the mutual contact of copper and zinc, the copper becomes negative, as to its electricity, and the zinc positive. The development of the electric fluid is consequently independent of any humid conductor. All the other facts connected with this leading one, the commission ascertained to be incontestible. The reporter next adverts to the mode in which the above facts are explained by VOLTA, who establishes between them and the principal one an intimate connection. He shews how easy it is, according to the theory of this great naturalist, to explain his pile; and, to do this with a greater degree of simplicity, supposes it to be fixed on an insulator. He represents by unity, the excess of electricity which a piece of zinc has over a piece of copper, when the two metals are brought into direct contact.

VOLTA likewise makes two suppositions, the first of which is, that the galvanic fluid is transmitted from one pair of plates to another, in the insulated pile, through the bits of moistened pasteboard, even when there does not exist any communication between the two extremities of the pile. The second supposition is, that the excess of electricity which the zinc takes from the copper, is, relatively to these two metals, invariable, whether they are, or are not, in their pure and natural state. VOLTA founds the first of these suppositions on an experiment, in which the condenser becomes charged, when the collecting plate, covered by a piece of wetted paper, is touched by the copper extremity of a metallic plate, the other extremity of which, made of zinc, is held between the fingers. "With respect to the second proposition," observes the reporter, "it is as simple as can possibly be imagined. It requires, however, he adds, a series of very nice experiments, which we have not as yet had an opportunity to make, to ascertain the precise degree in which it is conformable to Nature."

He afterwards observes, that the same theory is likewise applicable to any two metallic substances whatever of which the pile may be composed; and that the effects of the different forms which may be given to the apparatus employed for its construction, will depend on the differences of the electricity which may ensue, at the instant of contact. This equally holds good with respect to all other substances, between which there may subsist an analogous action. Thus, notwithstanding this action may in general appear very weak between fluids and metallic substances, there are still several of the former, such as the alkaline sulphurets, which, when combined with metals, have a very sensible action. In this way the English experimenters have succeeded in supplying one of the metallic elements of the pile by the above sulphurets. M. PFAFF has likewise applied them to this use in his experiments.

On this head, Professor VOLTA has discovered a very singular relation between metallic substances. by which the construction of a pile, with these substances alone, is rendered impracticable. It has been seen, in the analysis of the memoir itself, that VOLTA divides the conductors into two classes, the former of which comprehends solids, and the latter fluid bodies. It has been hitherto impossible to construct an apparatus in the form of a pile, in any other way than by a due and appropriate assemblage of conductors of the two classes above cited. That it is utterly impracticable with the former of them, has been fully demonstrated; but the reciprocal action of the substances of which the second class is composed, is not as yet sufficiently understood, to enable naturalists to decide whether this fact is equally applicable to them.

"Such is pretty nearly," observes the reporter, "the summary of the theory of VOLTA, relatively to the electricity which has been denominated galvanic. Its aim has been to reduce all the phenomena to a single one, the existence of which is now well established. It consists of the development of metallic electricity, by the mutual contact of metals. It seems to be proved, by the experiments of this naturalist, that the fluid to which the muscular contractions, and the phenomena of the pile, are ascribable, notwithstanding it has long been considered as a particular fluid—a fluid *sui generis*—is merely the common electric fluid, put in action by a cause the nature of which is unknown, but the effects of which we evidently see.

"After having found, and estimated, as it were, by approximation, the mutual action of the metallic elements, it remains that it should be ascertained in a precise manner, and that an inquiry should be made, whether it is fixed and invariable in metallic substances of the same nature, or whether, on the other hand, it varies with the qualities of the electricity they contain, as well as with their temperature. It is necessary to calculate, with an equal degree of precision,

cision, the appropriate action which fluid bodies have on each other, and on metallic substances. We shall then be enabled to form an estimate on exact *data*, and reduce the distribution and motion of the electricity, in the apparatus of Professor VOLTA, to the true law which they obey, thus completing the explanation of the phenomena the pile presents. These nice researches require, however, a recourse to the nicest instruments which naturalists have contrived, to measure the force of the electric fluid. Finally, it remains to inquire into the chemical effects of this electric current; into its action on the animal economy; and into its degree of similitude with the electricity of minerals and fishes. These researches cannot fail to be highly interesting."

The reporter, M. BIOT, concludes by a rapid view of the progress of electricity, from its origin to the discovery of the galvanic phenomena. These phenomena, he observes, which are stamped by so many singularities, and which are so different in appearance from whatever had been heretofore known, led to the matchless discovery of Professor VOLTA, and enabled him to apply them to the construction of an apparatus, calculated to augment their force at pleasure, and to connect them, by certain results, with other phenomena, of infinite importance both to chemistry and to the animal economy.

In three notes, by which the report is terminated, several of the phenomena of the Voltaic pile are subjected to a nice calculation. Several others, M. Biot observes, may be calculated in the same way; but, to the end that the *data* may be exact, it is necessary that the experiments should be made with the utmost precision.

CHAP. XV.

Interesting inquiries on galvanism, made by British philosophers since the discovery of the Voltaic pile.

NO sooner had the discovery of the pile been announced, in the letter, referred to in our last chapter, from Professor VOLTA to Sir JOSEPH BANKS, than it excited a general interest among the British philosophers, a summary of whose inquiries we now proceed to give. We begin by extracting the following article from TILLOCH's Philosophical Magazine*.

" Mr. A. CARLISLE, surgeon, having been favoured with a perusal of the above letter, by the very respectable President of the Royal Society, repeated, in conjunction with Mr. NICHOLSON, of Soho-square, the experiments of Professor VOLTA, and obtained similar results. At a very early part of the investigation, the pile having been formed, and the contacts secured by placing a drop of water upon the upper plate of the pile, Mr. CARLISLE observed a disengagement of gas round the conducting wire. This gas, though minute in quantity, seemed to Mr. NICHOLSON to have the smell of hydrogen, when the wire of communication was steel. This, with some other facts, led him to propose to break the circuit, by the substitution of a tube of water between two wires. Accordingly, a brass wire through each of two corks was inserted at the opposite ends of a glass tube about half an inch in diameter, filled between the corks with water: the distance between the points of the wires in the water, was about an inch and three quarters.

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* Vol. vii. p. 337.

"This compound discharger was applied so that the external ends of its wire were in contact with the two extreme plates of a pile of thirty-six half-crowns with the correspondent pieces of zinc and pasteboard. A fine stream of minute bubbles immediately began to flow from the point of the lower wire in the tube, which communicated with the silver, and the opposite point of the upper wire became tarnished, first deep orange, and then black. On reversing the tube, the gas came from the other point, which was now lowest, while the upper, in its turn, became tarnished and black. Reversing the tube again, the phenomena again changed their order. In this state the whole was left for two hours and a half. The upper wire gradually emitted whitish filmy clouds, which, towards the end of the process, became of a pea-green colour, and hung in perpendicular threads from the extreme half inch of the wire, the water being rendered semi-opaque by what fell off, and in a great part lay, of a pale green, on the lower surface of the tube, which, in this disposition of the apparatus, was inclined about forty degrees to the horizon. The lower wire, three quarters of an inch long, constantly emitted gas, except when another circuit, or complete wire, was applied to the apparatus; during which time the emission of gas was suspended. When this last mentioned wire was removed, the gas re-appeared as before, not instantly, but after the lapse of four beats of a half-second clock standing in the room. The product of gas, during the whole two hours and a half, was two-thirtieths of a cubic inch. It was then mixed with an equal quantity of common air, and exploded by the application of a lighted waxed thread.

" To have reversed the tube would have answered the same purpose, but they chose to do this, and found that, when the zinc was at the bottom, its effects were reversed; that is to say, the gas still came from the wire communicating with the silver, &c.

" Messrs. Carlisle and Nicholson were led, by reasoning on the first appearance of hydrogen, to expect a decomposition of water; but it was with no little surprize that they found the hydrogen extricated at the contact with one wire, while the oxygen fixed itself in combination with the other wire at the distance of almost two inches. As the distance between the wires formed a striking feature in this result, it became desirable to ascertain whether it would take place to greater distances. When a tube three quarters of an inch in diameter, and thirty-six inches long, was made use of, the effect failed, though the very same wires, inserted into a shorter tube, operated very briskly.

"The experiment being tried with tincture of litmus in place of water, and the oxydating wire, namely, from the zinc side, being lowest in the tube, it changed the tincture red in about ten minutes as high as the upper extremity of the wire. The other portion remained blue. Hence it seems either an acid was formed, or that a portion of the oxygen combined with the litmus, so as to produce the effect of an acid.

"It may be here offered as a general remark, that the electric pile with card, or with woollen cloth, continues in order for about two days, or scarcely three; that, from a series of glasses set up by Mr. Carlisle, as well as from the pile itself, it appears that the same process of decomposition of water is carried on between each pair of plates, the zinc being oxyded on the wet face, and hydrogen given out; that the common salt is decomposed, and exhibits an efflorescence of soda round the edges of the pile, extruded, most probably, by the hydrogen: and that, on account of the corrosion of the surfaces of the zinc, it is necessary to renew them previously to each construction of the pile. This may be done by scraping or grinding.

" By several accurate and well conducted experiments, Mr. NICHOLSON ascertained that the electricity of the zinc was plus, and that of the silver minus, whichever of them were at the top of the pile. The electric spark was even rendered visible; so that there can be no doubt of the identity of the electric and galvanic fluids.

" The decomposition of water, and oxydation of metallic wire, suggested other experiments. Two small wires of platina were inserted, as before, in a short tube. When the connexion with the pile was formed, the wire from the silver gave a plentiful stream of gas, and that from the zinc a smaller one. In four hours, neither turbidness, oxydation, nor tarnish appeared. The larger stream was naturally supposed to be hydrogen, the smaller oxygen.

" With thick gold leaf, instead of platina, the result was the same, only the extremity of the slip connected with the zinc acquired a coppery or purplish tinge.

"A brass wire was substituted for one of the slips of gold. When the former was joined to the silver end, the two streams were extricated as before; but when joined to the zinc, it became oxyded, as when both the wires were of brass.

"The simple decomposition of water by platina wires, without oxydation, offered a means of obtaining the gases separate from each other. This was tried with a pile of sixty-eight sets. A wire from each end of the pile passed under separate phials full of water inverted in a saucer of water. A cloud of gas arose from each wire, but most from the silver or minus side. Bubbles were extricated from all parts of the water, and adhered to the whole internal surface of the vessels. The process was continued for thirteen hours, after which the wires were disengaged, and the gases decanted into separate bottles. On measuring the quantities, which was done by weighing the bottles, it was found that the quantities of water displaced by the gases were, respectively, 72 grains by the gas from the zinc side, and 142 grains by the gas from the silver side; so that the whole volume of gas was 1.17 cubic inches, or near an inch and a quarter. These are nearly the proportions in bulk of what are stated to be the component parts of water. The gas from the zinc side being tried with one measure of nitrous gas, contracted to 1.25, and did not contract more by the addition of another measure; the gas from the silver side, by the same treatment, contracted to 1.6. The air of the room, on trial, contracted to 1.28. From the smallness of the quantity, no attempt was made to detonate the air from the zinc side; but a portion of that from the silver side, being mixed with one side of atmospheric air, gave a loud detonation.

" Upon the above it may be remarked, that it does not seem probable that oxygen was afforded by both wires, but that they were mixed by the circumstances of the experiment.

" Mr. Cruickshank, of Woolwich, also made some interesting experiments on this subject. He employed plates of zinc and silver about 1.6 inches square; and the number of each varied from 40 to 100, according to the power required. He used silver wire both from the zinc and silver plate in his first experiments; but to distinguish the ends of the pile, that wire only is called the silver wire, which was connected with the silver plate; the other wire he calls (to save circumlocution) the zinc wire. These wires were passed through corks fitted into a glass tube filled with water, and one of the corks made perfectly tight by means of cement. The tube was then placed upright in a cup containing water, with the uncemented end downwards.

As soon as the communication was made between the extremities of the pile by the wires, a quantity of small air-bubbles began to ascend from the end of the wire connected with the silver, as observed by Messrs. NICHOLSON and CARLISLE; but a white cloud at the same time made its appearance at the one proceeding from the zinc, or the zinc wire. This cloud gradually increased, and assumed a darker colour, and at last it became purple, or even black. A very few air-bubbles were likewise collected upon, and ascended from this wire; but when the machine was in full force, a considerable stream could be observed.

" The gas was collected, and found to be a mixture of hydrogen and oxygen, in the proportion of three parts of the former to one of the latter. No great dependence, however, was placed upon this in point of accuracy. The zinc wire was found to be much corroded, and looked as if a considerable portion of it had been dissolved. As the cloud which was formed around this wire became purple on exposure to the light, Mr. CRUICKSHANK suspected it might be luna cornea, or muriat of silver proceeding from the silver, which had been somehow dissolved, and afterwards precipitated in this state, by the muriatic salts in the common water.

" Distilled water, to which a little tincture of litmus was added, was next employed in the tube. Gas arose from both wires, but in the greatest quantity from the silver wire. In a short time, the whole fluid below the point of the zinc wire became red, and the fluid above the silver wire looked of a deeper blue than before, the slight tinge of purple being destroyed.

" Distilled water, tinged with Brazil wood, soon became of as deep a purple as could be produced by ammonia, while the portion of the fluid round the zinc wire became very pale. From these experiments it appears to Mr. Cruickshank, that an acid, probably the nitrous, is produced at the wire connected with the zinc, and an alkali, probably ammonia, at that connected with the silver end of the pile.

" When lime-water was employed, the wire was likewise acted upon, but in a less degree. The cloud at first had an olive colour, exactly resembling the precipitate of silver by lime-water.

" In these experiments the quantity of silver dissolved was considerable, and, where water was employed, a portion of it remained in solution, which was proved by adding muriatic acid. More would probably have been suspended, but that an evident precipitation near the upper extremity of the zinc wire, was occasioned by the alkali generated by the process.

" As hydrogen gas, when heated, or in its nascent state, reduces metallic oxyds, Mr. CRUICKSHANK filled the glass tube with a solution of acetite of lead, to separate the hydrogen from the oxygen, and thus obtain the latter pure. An excess of acid was added to the acetite to take up the alkali: in a minute or two after the communication was made, some fine metallic needles, which afterwards assumed the form of a feather, or rather that of the crystals of ammonia, were perceivable at the end of the silver wire. The lead was in its metallic state.

" Solutions of sulphat of copper and nitrat of silver, were tried in the same way, and with similar results. The metals were revived.

" When pure water, mixed with distilled vinegar, or with a very little sulphuric acid, were employed in the tube, metallic silver was precipitated by the silver wire, the acid employed preventing the alkali from precipitating the silver dissolved by the generated acid; in consequence of which, when a sufficient quantity of the metal was taken up, it was again thrown down by the silver wire in its metallic form.

" Muriat of ammonia in solution being tried, a little gas was disengaged from the silver wire: an incrustation of luna cornea was formed round the zinc wire. The liquor remaining after the experiment, smelled strongly of ammonia. Common salt was decomposed also. Indeed, when a solution of muriat of soda, or of ammonia, is employed to moisten the papers in the pile, the salt is always decomposed.

"Nitrat of magnesia was decomposed by the same means.

"In some after experiments gold wires were tried, and the quantity of oxygen gas obtained was much greater than when silver wires were employed.

"Two gold wires were passed through a cork loosely introduced into the mouth of a three-ounce phial filled with lime-water: the phial being inverted over pure water, the exterior ends of the wires were connected with the pile in the usual way. In four hours the phial was filled with gas extricated from the wires, especially the one connected with the silver. One measure of the gas being mixed with two of nitrous gas, a diminution of one measure took place: the residuum contained nitrous gas mixed with hydrogen. Four measures exploded by the electric spark over mercury, disappeared, except about one-fifteenth of a measure, which appeared to be azot.

"Two gold wires were passed through corks, secured by cement in the ends of a glass tube, about ten inches long, bent into the form of the letter V; they reached to within an inch of each other, at the angle, in which there was a hole about one-tenth of an inch in diameter. The tube was then filled with distilled water, and the opening at the angle being shut with the finger, to keep in the water, it was thus placed in a cup of water with the angle downwards. The extremities of the wires being then joined to those of the pile, gas was disengaged from both, but most from that connected with the silver; the gases were thus kept distinct. One measure of the gas from the silver end, mixed with one of nitrous gas, gave red fumes, a diminution of one-third of a measure, and a residuum consisting of nitrous and hydrogen gas. Two measures with one of oxygen being exploded over mercury, disappeared, except about one-fifth of a measure, which by the nitrous test appeared to be chiefly oxygen. A dense white vapour was perceived over the mercury for some time after the explosion. One measure of the gas from the zinc end, being mixed with two of nitrous gas, the whole nearly disappeared: another measure of the latter being added, the total diminution was nearly three measures.

"With platina wires Mr. CRUICKSHANK obtained almost similar results. The one connected with the zinc end became tarnished; the same thing happened when gold wires were used.

"A solution of crystallized muriat of lime, inclosed in a tube in the common manner, and gold wires, being employed, the one from the silver gave little gas, but that from the zinc a considerable quantity, and the fluid surrounding it assumed a fine yellow colour, a solution of the gold having been effected. After a time some gas came from the first wire, but there was no precipitation of lime. When the tube was opened, the fluid smelled of aqua regia, or the oxy-muriatic acid. When platina wires were employed in place of gold, the smell of nitro-muriatic acid was soon observable, but no solution of the platina. When the tube was filled with a solution of muriat of soda, a nitro-muriatic acid was likewise produced.

" Mr. Cruickshank from these experiments draws the following conclusions:

" 1. That hydrogen gas, mixed with a very small proportion of oxygen and ammonia, is somehow disengaged at the wire connected with the silver extremity of the machine; and that this effect is equally produced, whatever the nature of the metallic wire may be, provided the fluid operated upon be pure water.

" 2. That where metallic solutions are employed instead of water, the same wire which separates the hydrogen revives the metallic calx, and deposits it at the extremity of the wire in its pure metallic state; in this case no hydrogen gas is disengaged. The wire employed for this purpose may be of any metal.

" 3.

" 3. That of the earthy solutions, those of magnesia and argill only are decomposed by the silver wire; a circumstance which strongly favours the production of ammonia.

" 4. That when the wire connected with the zinc extremity of the pile consists either of gold or platina, a quantity of oxygen gas, mixed with a little azot and nitrous acid, is disengaged; and the quantity of gas thus obtained is a little better than one-third of the hydrogen gas separated by the silver wire at the same time.

" 5. That when the wire connected with the zinc is silver, or any of the imperfect metals, a small portion of the oxygenous gas is likewise given out, but the wire itself is either oxydated or dissolved, or partly oxydated and partly dissolved: indeed, the effect in this case, produced upon the metal, is very similar to that of the concentrated nitrous acid, where a great deal of the metal is oxydated, and but a small quantity held in solution *.

" 6. That when the gases, obtained by gold or platina wires, are collected together and exploded over mercury, the whole nearly disappears

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* The great difference in the effect produced by this influence on gold and silver, which have always been considered as equally difficult to oxydate, can only be explained on the supposition, that nitrous acid is generated; for this acid, it is well known, acts powerfully on silver, but has no action whatever on gold. The same observation applies to platina.

and ſorms water, with probably a little nitrous acid, for there was always a thick white vapour perceived for some time after the explosion. The residuary gas in this case appeared to be azot.

Acid solutions of metals having been decomposed, Mr. CRUICKSHANK tried their solution in alkalis. Pure ammonia was added to a dilute solution of nitrat of silver, till the mixture smelled strongly of the former. Being put into a tube in the usual way, with silver wires, and the communication made, a rapid production of gas took place from the silver end, but hardly any from the zinc. Greyish flashes of metallic silver were separated from the silver wire, and on the zinc wire a dark grey substance was deposited, which, on afterwards endeavouring to scrape it off with the finger, exploded, though still moist. The wire was corroded and full of holes. The fulminating silver of BERTHOLLET had in fact been produced in this experiment.

When pure ammonia was introduced into the tube in place of the solution of the nitrat of silver, the result was the same—the silver wire from the zinc being corroded, &c. was taken up by the alkali, and afterwards deposited in its metallic form by the other wire. To the first adhered some of the fulminating silver, and a portion was also deposited from the fluid, after standing some time.

From pure ammonia, with copper wires, a quantity of very pure metal was precipitated—from an ammoniacal solution of copper the same pure metallic precipitate. From these experiments Mr. CRUICKSHANK remarks, that it appears that the galvanic influence might be employed with success in the analysis of minerals.

Pure ammonia being introduced into a bottle, and inverted over the same fluid, with a cork and two wires, as before described (the wire connected with the zinc being platina and the other silver), a rapid decomposition of the alkali was effected. A two ounce measure of gas being collected and examined, was found to consist of 15 parts of hydrogen gas, 13 of azotic, and two nearly of oxygen gas.

The nitrous acid seemed to be little or not at all acted upon by the galvanic influence, from which Mr. CRUICKSHANK infers, that it is so perfect a conductor of the fluid, as to transmit it like metals, without experiencing any change; and this he thinks may possibly be owing to the great proportion of oxygen which enters into its composition, having before remarked that all fluids, containing little or no oxygen, are non-conductors, or nearly so.

The corroded matters generated at the wire connected with the zinc are not, Mr. CRUICKSHANK observes, pure oxyds; and it follows that an acid, as before mentioned, and probably the nitrous is produced; for all the green oxyds of copper contain an acid of some kind or other, the pure oxyds of that metal being either dark red or deep brown.

By the employment of a galvanic battery, consisting of several troughs* of his own invention, Mr. CRUICKSHANK succeeded in exploding a mixture of oxygen and hydrogen gases, by the means of the galvanic spark. Mr. BOLTON, of Birmingham, whose pile consisted of fifteen hundred pairs of plates, likewise produced sparks of an equal intensity.

Shortly after the Voltaic pile had been thus improved by Mr. CRUICKSHANK, several very interesting experiments were made by a body of scientific gentlemen in the capital, who assembled under the denomination of the Askesian Society.† The trough which was employed on this occasion, consisted of sixty pieces of silver, and an equal number of pieces of zinc, each of them two inches and a quarter square. The shock produced by this trough, by the means of two metallic conductors, was distinctly felt in the shoulders; the contraction, or spasm, being at the same time so violent, as to disable the operator from holding the conductors, when they were in contact with the plates, by which the trough was terminated at each side. A sensation resembling that produced by a highly heated fluid was likewise felt in the wrists and forearm.

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* The construction of these troughs will be described in the fourth and last part of this work.

† Phil. Mag. vol. x. p. 371.

A small piece of steel wire having been made a conductor, to unite the end pieces, on the contact being effected, a bright spark was observed, with small scintillations. Had it been in oxygen gas, the wire would, without a doubt, have deflagrated. On its being armed at the extremity with a small piece of phosphorus, the latter inflamed as soon as the contact was made.

A connexion having been established between the trough and a galvanometer of the invention of the ingenious Mr. PEPYS, upon moving the regulating screws, the gold leaves, at the distance of half an inch, began to diverge. One of them struck the zinc, and instantly inflamed, burning with a bright flame of a white colour. A sound was heard, as if a small quantity of nitre had been thrown on ignited charcoal; and about a quarter of an inch of the gold was destroyed, leaving a stain, of the colour of smoke, on the plate of zinc. This experiment was repeated several times with a similar result.

A similar trough, consisting of sixty pieces of silver and zinc, an inch and a half square, having been connected, in the order of arrangement, with the above-mentioned trough, evidently decreased its powers, so as evidently to demonstrate, that the union of weaker troughs with more powerful ones, is not attended by any advantage, as in the case of electrical batteries.

Professor PICTET, of Geneva, Messrs. TILLOCH and AIKIN, and several other scientific gentlemen, were present at the above experiments. At a subsequent meeting, the battery which was employed consisted of two troughs, each containing sixty plates of silver, and an equal number of zinc. There were consequently a hundred and twenty pairs of metallic plates, each of them two inches and a quarter square, or containing five square inches and one sixteenth of surface.

By the means of this powerful battery, gold was deflagrated with great facility. Thin leaves of that metal having been connected with one of the extremities of the battery, inflamed as soon as they were, to complete the circuit, brought in contact with the exterior plate of the other extremity. The combustion was entirely confined to the points in which the gold leaf and the plates were in contact; and would have ceased altogether, if the gold had not been constantly made to advance towards the plate, to supply, by fresh portions brought into contact,

tact, the place of those which had been deflagrated. It was deemed extraordinary that neither an oxyd, nor a residuum of any description, could be discovered. It is probable that the gold was volatilized by the heat by which its deflagration was occasioned. This point was reserved by the Society for future experiments, to be made with a larger portion of gold. In the mean time, in the same way as phosphorus had been inflamed at the former meeting, so did the society now succeed in exploding gunpowder, by a persistence in the same means.

Several interesting experiments on the deflagration of metals, were afterwards made by a society of gentlemen,* zealous promoters of science. A pile, consisting of thirty-six pairs of plates, of silver and zinc, between which were interposed disks of flannel, moistened with a solution of muriate of ammonia, was constructed. Each of the plates had a diameter of ten inches, or, in other words, contained 78.58 square inches; consequently, the entire surface of silver in the pile, reckoning one side only of each plate, was 2827.44 square inches; and that of zinc the same. Such an undertaking could not be accomplished without a very considerable expence.

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* In the number was Mr. Tilloch, the learned editor of the Philosophical Magazine, from which this account is extracted.

With this pile, in the month of December, 1801, gold, silver, copper, tin, lead, and zinc were deflagrated with a surprizing facility.

The gold burned with a very vivid white light, inclining somewhat to blue. This experiment afforded an opportunity to decide on a point which was left undetermined in the account of the meeting of the Askesian Society, precedingly given, and in which it was stated, that the gold was deflagrated without any residuum being perceived. On the present occasion, there was left on the upper plate a copious oxyd of a deep brown colour, inclining a little to purple. In the former experiment, the end-plate of the trough, to which the gold was applied, was vertical; and as the quantity of oxyd could have been but very small, at the same time that it was nearly of the colour of the mahogany on which it fell, its not having been perceived may be readily accounted for.

The silver gave a vivid flame of a greenish hue, and extremely brilliant. The colour was somewhat like that of a pale emerald, and the light more intense than that from the gold. Its oxyd was of a blackish hue.

The copper presented phenomena similar to those which attended the deflagration of the gold.

The lead gave a very vivid light, of a dilute bluish purple.

The tin afforded a light similar to that of the gold, but burned with less energy, probably on this account, that the leaves were thicker.

The zinc gave a bluish white flame, which was edged at the moment of contact with red. It was more difficult of deflagration than any of the preceding metals; but the plates were at the same time much thicker.

The oxyds of the last four metals were not examined.

Water having been poured on the upper plate, so as to form a kind of little standing pool, several of the metals were presented to the plate through this aqueous medium, and were deflagrated. They afforded a flame of the same colour as when they were brought to the bare plate. A vapour was sometimes perceptible instantly after the deflagration, and was conjectured to arise from a portion of the water having been converted into steam by the intense heat.

One particular circumstance was very deserving of remark, namely, that the shocks from this pile, notwithstanding it was in a state to produce on the metals such very powerful effects, could be received with a very trifling inconvenience.

While these very interesting experiments were making in the metropolis, Mr. William Henry, of Manchester, whose celebrity as a chemist had been long established, made several others, the result of which is equally curious and important. They were principally directed to acid and alkaline substances, subjected to the action of the Voltaic pile. The more prominent results are as follows:

Relatively to the decomposition of water by the galvanic influence, Mr. Henry had but little to add to the facts which were already known. He observes, that "if the water be confined by mercury, in a tube with a conductor sealed hermetically into one end, a production of gas will only take place when the conductor communicates with the silver side of the apparatus; and that, if the order be inverted, that is, if the mercury be connected with the silver end, and the conductor be stationed at the sealed extremity of the tube of the pile, no gas whatever will be produced, although the agitation of the surface of the mercury in the tube shews that the influence is still transmitted. On passing a wire of any metal through the quicksilver, so that its extremity may rise into the water, gas then begins to ascend copiously from the lower wire." Having made these observations, Mr. HENRY gives it as his opinion, that pointed bodies are more effectual in decomposing water, than bodies with rounded surfaces.

In subjecting concentrated sulphuric acid to the galvanic influence, he employed the following process. The acid was contained in a glass tube, provided with two platina conductors; and the open end of the tube was immersed in a cupful of the same acid. One half of the gas, which was thus produced in great plenty, was absorbed by the sulphuret of potash; while the remainder consisted of hydrogenous gas, from the decomposition of water, which the strongest sulphuric acid necessarily contains. As, however, the oxygenous gas would have sufficed to have saturated twice the quantity of the hydrogen gas which was evolved, it would appear that the one half of the former must have originated from another source than that of water, and was to be ascribed to the decomposition of the acid. In proof of this, the wire from which the gas ascended was surrounded by a white cloud, which was probably formed by dis-oxygenated sulphur.

Pure and colourless nitric acid underwent a rapid decomposition, and assumed a straw colour. The gas which was evolved consisted of the oxygenous and azotic gases, in the proportion of more than three parts of the former to one of the latter. Liquid muriatic acid gave out a gas, consisting nearly of two parts of oxygen, and one of hydrogen gas, both of which appear to have been occasioned by the decomposition of water.

A tube having been prepared with platina conductors, covered with wax, and having been filled with a saturated solution of oxygenated muriatic acid in water, the circuit was completed through it, between the two ends of the pile. The gas which was evolved, was a mixture of oxygen and hydrogen, in the proportions of a hundred and thirty-six to a hundred and eighteen. As the proportion of the latter gas required fifty-nine parts, by measure, of oxygen gas for saturation, the remaining seventy-seven parts of the oxygen must have arisen from the dis-oxygenation of the acid.

To ascertain whether the galvanic influence can be transmitted through the gases, a portion of phosphorated hydrogen gas was exposed over mercury, in a tube supplied at one end with a platina conductor, which was connected with the silver end of the pile, while the mercury by which the gas was confined, was connected with the zinc end. After it had stood for several hours, no change was observed to have ensued. Mr. Henry next exposed a mixture of muriatic acid and oxygenous gases, which are rapidly diminished by electrical discharges; but without perceiving any contraction of bulk, or change of properties. He was at first led to believe that a contraction had taken place, in consequence of the sealing-wax, by which the platina wires were covered, having absorbed a portion of muriatic acid gas. He observes, that the deficiency of the property of transmission through the gases, considerably limits the employment of galvanism as a chemical agent; and regrets that it overturned his project of attempting, by the intervention of the galvanic fluid, the analysis of the muriatic acid, relative to the composition of which no certain conclusions can be drawn, from experiments made on it when condensed with water.

Having subsequently found that it was impracticable to transmit the galvanic influence through ammoniacal gas, he exposed to its action a portion of water perfectly saturated with that alkali, and was not a little surprized at the result, which was confirmed to him by repeated experiments. As a proof that no portion of oxygenous gas was produced, the evolved gas was not produced by sulphuret of potash; neither did it inflame when an electric spark was made to pass through it. On firing it with oxygen gas, it became greatly diminished; and a solution of sulphuret of potash, after having abstracted the excess of oxygen gas, left a small bubble only of azotic gas, which was to be traced to the oxygen gas employed to effect the combustion. This fact was extremely singular and curious. The volatile alkali, Mr. Henry observes, was certainly decomposed; seeing that, if the hydrogen gas had proceeded from the decomposition of water only, oxygenous gas would also have been obtained. "Into what new combination," he inquires, "does the azot enter in this case? It is not improbable that, at the same instant, both water and ammoniac are decomposed; that the hydrogen of both is converted into gas; and that the oxygen of the water, uniting with the azot of the alkali, composes nitric acid, which, combining with the ammoniac, produces nitrate of ammonia. The destruction of the vegetable alkali is not less certain."

The result of his next, and final experiment, is equally interesting. Having exposed, in a similar way, a solution of caustic vegetable alkali, the gas produced was pure hydrogen gas. During the process, the surface of the mercury became covered with a blackish film, more particularly at its edges, where it was in contact with the glass tube. The black matter was, however, confined to the surface of the mercury, and was generated at the same part of the tube at which gas was produced, namely, at the part in contact with the platina wire. It still occurred, even on the use of mercury which had been carefully distilled for the purpose. "From this experiment," Mr. Henry observes, "a sufficient proof is obtained that the vegetable alkali contains hydrogen. Azot is probably another of its constituent parts, but is prevented from appearing in its gaseous state by its union with oxygen, evolved, at the same instant, by the decomposition of a portion of water. These suggestions admit of being verified by a careful examination of the alkalis, after an exposure to the influence of galvanism; and a third component of the vegetable alkali will perhaps be found in the black precipitate above described. I have not been inattentive, even hitherto, to these points; but the minuteness of the quantities submitted to experiment, prevents me from speaking decisively, and I choose rather to reserve myself for the results of the experiments I am now making, than to incur the risk of being forced to retract a hasty assertion."

On a subsequent examination, Mr. Henry found that the black precipitate proved to be merely a metallic oxyd, and not charcoal, as he had at first been led to suppose. By varying the experiment, and transmitting the galvanic influence through liquid caustic alkali, without the contact of mercury, the black powder no longer made its appearance. The gases proved to be a mixture of hydrogen and oxygen, pretty nearly in the proportions which might have been expected from water. The imperfect metals contained in the quicksilver, had hitherto prevented the oxygen from assuming a gaseous form.

While the galvanic experiments were multiplying in great Britain, the celebrated Dr. PRIESTLEY repeated them on the American continent. He had for that purpose received from Birmingham, an apparatus for the construction of a Voltaic pile, consisting of sixty plates of silver coated with copper, and as many thin rolled plates of zinc. He made a variety of interesting experiments, in the course of which he observed several facts which had been already noticed in England, but without his knowledge. In one of these experiments he interposed, between the two ends of the pile, four glasses of water, which were connected by the means of silver wires, at the extremities of which the usual processes went on. It was found, however, that when one of the portions of water had its surface covered with oil, the action was suspended in each of the glasses. This experiment, when repeated, did not succeed in England.

In these trials the Doctor had constantly in view his favourite hypothesis of phlogiston, which he endeavoured to establish and confirm. He observes, that he cannot bring himself to consent to the modern doctrine of the decomposition of water, which, however generally it may have been received, he considers as altogether chimerical, and unable to stand its ground for any length of time. In adverting to the galvanic experiments which had been made with the Voltaic pile, he remarks that they do not, in general, furnish any support to the above doctrine; and, in proof of this, he adduces the following reasons: "Although," he observes, "it may happen that the inflammable air from the wire connected with the silver end of the pile, may be in the proportion to the *dephlogisticated* air from the wire connected with the zinc end, which the hypothesis of the decomposition of water requires, it appears that the latter proceeds from the air which is merely held in solution in the water in which the process is made; since if, by the means of oil upon the water, or of a vacuum, access to the atmosphere be cut off, the production of air entirely ceases. In the same way, there is not any production of air when the water has been exhausted of it; and certainly no good reason can be given, why, if the water itself consists of these two kinds of airs, and this process be capable of decomposing it, air should not be produced from it in all these cases; both the constituent parts of the water being present, and the power of separating them being in full operation. Besides, I find that, although the two kinds of air be produced, they are not always in the proportion required by the new theory, the dephlogisticated air being much less than is requisite. I have also found it to be not much better than atmospherical air. The inflammable air I believe to be of the purest kind."

After a recapitulation of several other facts, by which he endeavours to support his hypothesis, and, among which, he particularly notices that, unless gold, or platina, be connected with the zinc end of the pile, there is seldom any production of air from that quarter, the metal from that situation being dissolved; and that there is no appearance of its being dissolved by any *acid*, but, on the contrary, of its being supersaturated with phlogiston, he proceeds to give his own particular opinion relative to the theory of the phenomena of the Voltaic pile. It is as follows:

"Since *," he observes, "the operation wholly depends on the calcination of the zinc, which suffers a great diminution of weight, while the silver is but little affected; and as all metals lose their phlogiston in calcination, what remains of the zinc in a metallic form in the pile, and every thing connected with that end of it, is supersaturated with phlogiston, while the calcined part, and every thing connected with that end of the pile, is deprived of it. The former, therefore, is in a *positive* state, and latter in a *negative*, with respect to phlogiston; and it seems to follow from these experiments, that this is the same thing with positive and negative electricity; so that the electric fluid and phlogiston are either the same, or have some near relation to each other. The silver seems to act principally as a conductor of electricity; for the surface of it is merely blackened in some places in this process, probably in consequence of receiving phlogiston from the zinc; but the water is most essential to it, because it constitutes the principal part, if not the whole, of the addition of weight in the calx. Accordingly, in the calx of zinc I have found nothing but water, although it is probable that it contains a small portion of oxygen.

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* Nicholson's Journal, vol. i. 8vo. p. 202.

"These experiments favour the hypothesis of two *electric fluids*, the positive containing the principle of oxygen, and the negative that of phlogiston. These, being united to water, seem to constitute two opposite kinds of air, namely, dephlogisticated and inflammable.

" They tend likewise to confirm the conjecture which I advanced in my first publication on the subject of air, concerning *the similarity of the electric matter to phlogiston*; and, together with the purely galvanic experiments, shew, that the same substance, elaborated from the aliment by the brain, is the cause of muscular motion, the nerves being the most sensible of all electrometers *.

" I see no occasion to suppose, with M. Volta, that there is any circulation of the electric fluid in this pile. The calcination of the zinc supplies phlogiston as long as it continues, and when that ceases, the operation of the pile ceases with it. I also do not see the necessity why one end of the pile should be silver, and the other zinc; seeing that, when both are silver, or both zinc, the operation is the same; neither can I conceive why it should be otherwise. When the pile is properly prepared, the addition of any kind of metal to the ends only serves as a conductor of the electric fluid; and silver, zinc, or any other metal, will sufficiently answer this purpose.

" Had this process succeeded without any atmospherical air incumbent on the water in which it is made, it would have amounted to a full proof of the new theory, one part of the water being deprived of hydrogen, while oxygen abounded in the other, and both of them, with the assistance of *caloric* (though it does not appear whence that could be supplied) assuming the form of air. But this not being the case, the element of the dephlogisticated air evidently coming from the superincumbent atmosphere, the element of the inflammable air must necessarily arise from the calcined metals, which is a sufficient proof of the doctrine of phlogiston."

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* Priestley's Experiments on Air, first edition, vol. i. p. 274, et sequent.

We have been the more anxious to give the above theory of the phenomena of the Voltaic pile, because, however the doctrine on which it is founded may have given place to new hypotheses, it has still its partisans and admirers. In a postscript to the above communication, the Doctor claims the discovery of the conducting power of charcoal, which had been ascribed to the inventor of the galvanic pile, observing that it was one of his first observations in electricity, made in 1766, and published in his History of Electricity in the following year. He subjoins a second postscript, in which he observes that, having covered the whole of the pile with a large receiver, standing in water, charcoal being connected with the silver end of the pile, and silver with the zinc end, in two vessels of water, the solution of the silver took place in both the vessels, and the air within the receiver began to diminish. The diminution having come to its maximum in about a day and a half, Dr. PRIESTLEY examined the air within the receiver, and found it completely phlogisticated, being not in any degree affected by nitrous air. There was evidently, therefore, he observes, no generation of dephlogisticated air, the result being entirely the effect of the calcination of the zinc. This experiment is, according to him, a full and sufficient evidence that, in this case, there is no decomposition of water; and it thus tends to strengthen the argument in favour of the doctrine of phlogiston.

Mr. DAVY, professor of chemistry at the Royal Institution, has been one of the most zealous promoters of the galvanic science. Shortly after the discovery made by VOLTA, he instituted a course of experiments on the galvanic influence, from which several new and interesting observations resulted. The apparatus he employed never consisted of less than a hundred and ten pairs of metallic plates. He found that the galvanic shock was considerably increased, when the parts communicating with the conductors were moistened with a solution of green sulphate of iron. A pile, with pieces of cloth wetted with that substance, acted rather more intensely than a similar pile constructed in the usual manner. It, however, lost its powers in a shorter time.

To determine whether the oxygen and hydrogen gases, evolved by the interrupted wire when immersed in water, could be procured from two portions of water kept distinct from each other, he caused the wires to terminate in two glasses of water, between which he established a communication with his fingers. The gases were produced as before. When the water had been very carefully boiled, they were evolved in a nearly pure state, and in the proportions requisite to the formation of water. To ascertain whether it was essential that the wires should be in contact with the metallic plates of the pile, its extremities were made to communicate with two glasses of water, connected by a silver wire, by the means of pieces of muscular fibre. The effects of the pile were now reversed: the hydrogen was disengaged by the wire at the zinc end, while the wire at the silver extremity became oxydated.

The next inquiry made by Mr. DAVY was into the influence of the atmosphere on the phenomena of galvanism. To ascertain the effect of the factitious airs on these phenomena, his piles were constructed horizontally, in the usual manner; but, to prevent the plates from separating, when in an oblique position, he joined their sides together, at two or three points, by a resinous cement, leaving a sufficient interstice to admit of a free circulation of air. When there was any production of the gases, they were received in small tubes filled with distilled water, and containing wires, externally covered with wax, which communicated with the extremities of the pile. The piles were introduced into the airs through water, and elevated above the water by a metallic plate cemented to their lower extremities. Mr. DAVY had previously ascertained, by numerous trials, that a pile acts in the atmosphere, immediately after its immersion in water, without being wiped, although more feebly than before. He had likewise found that, after the first immersion, the powers were not diminished by the subsequent ones.

The following inferences were drawn by Mr. DAVY from these experiments.

1st, That zinc, whether connected with silver in single galvanic circles, or constituting the plates of the Voltaic pile, does not appear to undergo any oxydation at common temperatures, so long as the water in contact with it is pure.

2dly, That the oxydation of the zinc plates of the galvanic pile takes place whenever the water in contact with them holds in solution either atmospherical air, oxygen, nitrous gas, nitrous acid, marine acid, &c.

3dly, That when the zinc, in contact with water holding in solution substances containing loose oxygen, or acids, is oxydated, these substances are changed, or, in other words, exert certain chemical affinities.

4thly, That the Voltaic pile appears to be incapable of acting when the water between the pairs of plates is pure.

5thly, That it acts when the water between the double plates hold in solution either asmospherical air, oxygen, nitrous gas, nitrous acid, or marine acid.

6thly, and lastly, That the power of the action of the Voltaic pile appears to be proportionate to the power of the fluid conducting substance, between the double plates, to bring about the oxydation of the zinc.

The following general conclusions are made by Mr. DAVY. That, "of two phenomena, or of two series of phenomena, we can only affirm that the one is the cause of the other, when it uniformly precedes it, and when their modifications are connected. But it appears from all the foregoing facts, that the galvanic pile of VOLTA acts only when the conducting substance between the plates is capable of oxydating the zinc; and that, in proportion as a greater quantity of oxygen enters into combination with the zinc in a given time, so in proportion is the power of the pile to decompose water, and to produce a greater shock. It seems therefore reasonable to conclude, although, with our present quantity of facts, we are unable to explain the exact mode of operation, that the oxydation of zinc in the pile, and the chemical changes connected with it, are *some how* the cause of the electrical effects it produces."

The conducting power of charcoal has been repeatedly noticed in this work. It was found by Mr. Davy to possess the same properties as metallic substances, in the production of the shock and spark, when it became a medium of communication between the extremities of the galvanic pile. He likewise found that charcoal, perfectly well made, when connected, in the galvanic circuit, with water or aqueous solutions, effects changes in them analogous to those produced by metals, but attended, at the same time, by peculiar appearances.

In these trials the apparatus decomposed water in the usual manner, the silver end giving out hydrogen with a little carbon in solution, while the zinc end evolved but little gas of any description. This was probably owing to the absorption of the carbonic acid, which must have been formed at the end of the wire. Mr. Davy afterwards constructed a series of eight glasses, with small pieces of well burnt charcoal, zinc, and a solution of red sulphate of iron, the charcoal and zinc being connected by silver wires. This series gave sensible shocks, and occasioned a rapid a rapid evolution of gases from water; while an equal series, with silver and zinc, produced much weaker effects. He infers that charcoal and zinc are equal, if not superior, to any metallic combinations.

Mr. DAVY's next inquiry was, whether the difference of metals was essential to the effect produced by the Voltaic pile. He found that this pile may, by the application of proper fluids to its surfaces, be constructed with one metal only. This observation was likewise made by Professor GREN. The following series appears to have been employed by Mr. DAVY: metal, nitrous acid, water, sulphuret of potash, and, lastly, metal. The metals, with each of which an attempt was made to form a distinct pile, were silver, copper, zinc, and lead. It was noticed that the effects of a pile of this description were the reverse of those which occurred, when the difference of action depended on the metals; or, in other words, that the extrication of hydrogen, in the tube of communication, was made from the wire opposite to the one from which it would have been made with the common pile. On the subject of this interesting experiment, it is very judiciously observed by Mr. NICHOLSON, that "among the reflections and conclusions to which it must give rise, it is not one of the least important, that it overthrows the theory of the learned inventor VOLTA, who considers the current of electricity as being produced by the difference of conducting power in the different metals. In this obscure region of research, he adds, although we are altogether unacquainted with the power by which the electricity is accumulated, it is one step towards a true theory, to have ascertained, that one of those already offered is without foundation."

In the month of June 1801, a very interesting paper, by Dr. HYDE WOLLASTON, F.R.S. on the identity of the galvanic and electric fluids, was read before the Royal Society. By this paper it appeared that the Doctor had succeeded in decomposing water by the means of common electricity, as rapidly as by the pile of VOLTA. He had obviated the difficulty which was caused by the fluid passing, in a greater or less degree, in the form of sparks, by employing a gold wire, and a silver wire, insulated in the opposite ends of a glass tube, and presenting to each other as fine a point as they could possibly be made to receive. The tube in which they were inserted contained the water to be decomposed, and constituted a part of an electric circuit.

The Doctor afterwards communicated to the Society, a paper containing the results of a variety of experiments on the chemical production and agency of electricity. As this paper tends to illustrate, in a most satisfactory manner, the primary cause of the galvanic and electric phenomena, of the decomposition of water, &c. we insert it at full length.

" Notwithstanding the power of M. Volta's electric pile is now known to be proportional to the disposition of one of the metals to be oxydated by the fluid interposed, a doubt has been entertained by many persons, whether this power arises from the chemical action of the fluid on the metal; or, on the contrary, whether the oxydation itself may not be occasioned by electricity, set in motion by the contact of metals that have different conducting powers.

" That the oxydation of the metal is the primary cause of the electric phenomena observed, is, I think, to be inferred from the following experiments, which exhibit the galvanic process reduced to its most simple state.

" *Experiment* I. If a piece of zinc and a piece of silver have each one extremity immersed in the same vessel, containing sulphuric or muriatic acid diluted with a large quantity of water, the zinc is dissolved, and yields hydrogen gas, by the decomposition of the water: the silver, not being acted upon, has no power of decomposing water; but, whenever the zinc and silver are made to touch, or any metallic communication is made between them, hydrogen gas is also formed at the surface of the silver."

Any other metal beside zinc, which, by the aid of the acid employed, is capable of decomposing water, will succeed equally, if the other wire consists of a metal on which the acid has no effect.

" *Experiment* II. If zinc, iron, or copper, be employed with gold, in dilute nitric acid, nitrous gas will be formed, in the same manner, and under the same circumstances, as the hydrogen gas in the former experiment.

" *Experiment* III. Experiments analogous to the former, and equally simple, may also be made with many metallic solutions. If, for instance, the solution contains copper, it will be precipitated by a piece of iron, and appear on its surface. Upon silver merely immersed in the same solution, no such effect is produced; but as soon as the two metals are brought into contact, the silver receives a coating of copper.

" In the explanation of these experiments, it is necessary to advert to a point established by means of the electric pile.

" We know that when water is placed in a circuit of conductors of electricity, between the two extremities of a pile, if the power is sufficient to oxydate one of the wires of communication, the wire connected with the opposite extremity affords hydrogen gas.

"Since the extrication of hydrogen, in this instance, is seen to depend on electricity, it is probable that, in other instances, electricity may be also requisite for its conversion into gas. It would appear, therefore, that in the solution of a metal, electricity is evolved during the action of the acid upon it; and that the formation of hydrogen gas, even in that case, depends on a transition of electricity between the fluid and the metal.

"We see, moreover, in the first experiment, that the zinc, without contact of any other metal, has the power of decomposing water; and we can have no reason to suppose that the contact of the silver produces any new power, but that it serves merely as a conductor of electricity, and thereby occasions the formation of hydrogen gas.

"In the third experiment also, the iron by itself has the power of precipitating copper, by means, I presume, of electricity evolved during its solution; and here likewise the silver, by conducting that electricity, acquires the power of precipitating the copper in its metallic state.

"The explanation here given receives additional confirmation from comparative experiments which I have made with common electricity; for it will be seen that the same transfer of chemical power, and the same apparent reversion of the usual order of chemical affinities in the precipitation of copper by silver, may be effected by a common electrical machine.

"The machine with which the following experiments were conducted, consists of a cylinder 7 inches in diameter, with a conductor on each side 16 inches long, and 3½ inches diameter, each furnished with a sliding electrometer, to regulate the strength of the spark received from them.

"*Experiment* IV. Having a wire of fine silver, $\frac{1}{120}$ of an inch in diameter, I coated the middle of it, for 2 or 3 inches, with sealing-wax, and, by cutting through in the middle of the wax, exposed a section of the wire. The two coated extremities of the wire, thus divided, were immersed in a solution of sulphate of copper placed in an electric circuit between the two conductors; and sparks, taken at $\frac{1}{10}$ of an inch distance, were passed by means of them through the solution. After 100 turns of the machine, the wire which communicated with (what is called) the negative conductor, had a precipitate formed on its surface, which, upon being burnished, was evidently copper; but the opposite wire had no such coating.

"Upon reversing the direction of the current of electricity, the order of the phenomena was of course reversed; the copper being shortly re-dissolved by assistance of the oxydating power of positive electricity, and a similar precipitate formed on the opposite wire.

" *Experiment* V. A similar experiment made with gold wires $\frac{1}{100}$ of an inch diameter, in a solution of corrosive sublimate, had the same success.

" The chemical agency, therefore, of common electricity is thus proved to be the same with the power excited by chemical means; but, since a difference has been observed in the comparative facility with which the pile of VOLTA decomposes water, and produces other effects of oxydation and de-oxydation of bodies exposed to its action, I have been at some pains to remove this difficulty, and can at least produce a very close imitation of the galvanic phenomena by common electricity.

" It has been thought necessary to employ powerful machines and large Leyden jars for the decomposition of water; but, when I considered that the decomposition must depend on duly proportioning the strength of the charge of electricity to the quantity of water, and that the quantity exposed to its action at the surface of communication depends on the extent of that surface, I hoped that, by reducing the surface of communication, the decomposition of water might be effected by smaller machines, and with less powerful excitation, than have hitherto been used for that purpose; and in this hope I have not been disappointed.

" *Experiment* VI. Having procured a small wire of fine gold, and given it as fine a point as I could, I inserted it into a capillary glass tube; and, after heating the tube so as to make it adhere to the point, and cover it in every part, I gradually ground it down, till, with a pocket lens, I could discern that the point of the gold was exposed.

" The success of this method exceeding my expectations, I coated several wires in the same manner, and found, that when sparks from the conductors before mentioned were made to pass through water, by means of a point so guarded, a spark passing to the distance of one-eighth of an inch would decompose water, when the point exposed did not exceed $\frac{1}{700}$ of an inch in diameter. With another point, which I estimated at $\frac{1}{1500}$, a succession of sparks, one-twentieth of an inch in length, afforded a current of small bubbles of air.

" I have since found that the same apparatus will decompose water with a wire one-fortieth of an inch diameter, coated in the manner before described, if the spark from the prime conductor passes to the distance of four-tenths of an inch of air.

" *Experiment* VII. In order to try how far the strength of the electric spark might be reduced by a proportional diminution of the extremity of the wire, I passed a solution of gold in *aqua regia* through a capillary tube, and, by heating the tube, expelled the acid. There remained a thin film of gold lining the inner surface of the tube, which, by melting the tube, was converted into a very fine thread of gold, through the substance of the glass.

" When the extremity of this thread was made the medium of communication through water, I found that the mere current of electricity would occasion a stream of very small bubbles to rise from the extremity of the gold, although the wire, by which it communicated with the positive or negative conductor, was placed in absolute contact with them. Hence it appears, that the decomposition of water may take place by common electricity as well as by the electric pile, although no discernible sparks are produced.

" The appearance of two currents of air may also be imitated by occasioning the electricity to pass by fine points of communication on both sides of the water; but, in fact, the resemblance is not complete; for in every way in which I have tried it, I observed, that each wire gave both oxygen, and hydrogen gas, instead of their being formed separately, as by the electric pile.

"I am inclined to attribute the difference in this respect to the greater intensity with which it is necessary to employ common electricity; for, that positive and negative electricity, so excited, have each the same chemical power as they are observed to have in the electric pile, may be asertained by other means.

"In the precipitation of copper by silver, an instance of de-oxydation (or phlogistication) by negative electricity has been mentioned: the oxydating power of positive electricity may be also proved by its effect on vegetable blue colours.

"*Experiment* VIII. Having coloured a card with a strong infusion of litmus, I passed a current of electric sparks along it, by means of two fine gold points, touching it at the distance of an inch from each other. The effect, as in other cases, depending on the smallness of the quantity of water, was most discernible when the card was nearly dry. In this state, a very few turns of the machine were sufficient to occasion a redness at the positive wire, very manifest to the naked eye. The negative wire, being afterwards placed on the same spot, soon restored it to its original blue colour.

" By M. VOLTA's apparatus, the same effects are produced in much less time.

" Beside the similarity which has thus been traced between the effects of electricity excited by the common machine and those observed from the electric pile, I think it appears also probable that they originate from the same source.

" With regard to the latter, its power is now known to depend on oxydation ; so also does the excitement in the former appear very much to depend on the same process ; for, by

" *Experiment* IX. I have found, that, by using an amalgam of silver, or of platina, which are not liable to be oxydated, I could obtain no electricity. An amalgam of tin, on the contrary, affords a good degree of excitement. Zinc acts still better ; but the best amalgam is made with both tin and zinc, a mixture which is more easily oxydated than either metal separately.

" *Experiment* X. But, as a further trial whether oxydation assists in the production of electricity, I mounted a small cylinder, with its cushion and conductor, in a vessel so contrived that I could at pleasure change the contained air.

" After trying the degree of excitement in common air, I substituted carbonic gas, and found that the excitement was immediately destroyed,

stroyed, but that it returned upon re-admission of atmospheric air.

"In conformity to this hypothesis, we find that the metal oxydated is, in each case, in a similar state of electricity; for the cushion of the machine, by oxydation of the amalgam adhering to it, becomes negative; and, in the same manner, zinc oxydated by the accumulated power of an electric pile, or simply by action of an acid, is also negative.

"This similarity in the means by which electricity and galvanism appear to be excited, in addition to the resemblance that has been traced between their effects, shews that they are both essentially the same, and confirms an opinion that has already been advanced by others, that all the differences discoverable in the effects of the latter, may be owing to its being less intense, but produced in much larger quantity."

The discovery of the Voltaic pile had been no sooner announced in Great Britain, than Colonel Haldane, whose philosophical investigations were already well known to the learned world, made several interesting experiments on the galvanic influence, which he was led to conclude, from the results he obtained, to be dissimilar to electricity. He appears, however, to have admitted, that a certain portion of electricity may have been generated and disengaged, during the operation of the Voltaic apparatus.

He tried the combinations of the different metals, and found that zinc acts perfectly well with gold, tin, lead, iron, and copper. According to him, iron also acts with the other metals above cited, as does also lead, although feebly. When he employed iron and silver, the oxydation took place at the wire connected with the iron, and the gas flowed from that connected with the silver. When zinc and iron, which acted very powerfully, were employed, the contrary occurred, seeing that in this latter case, the gas was produced by the wire connected with the iron. No other combinations than those referred to above would succeed, with this exception, however, that tin and gold afforded a very faint cloud in the water. It was deserving of remark that, in another experiment, the oxyd of copper wire deposited in the tube, was of a dusky coloured green, very different from that produced by the apparatus of zinc and silver, as if some portions of the metals of which the apparatus was composed, had entered into the circulation, and affected the colour of the oxyd. The Colonel observed, that the effects of the Voltaic apparatus were suspended when it was immersed under water, and that this was also the case when it was placed under the vacuum of an air-pump.

In the former of these instances, on taking it out, and wiping carefully its external surface, without separating the parts, it acted as well as before. Hence Mr. NICHOLSON is led to conclude*, that the power of the apparatus employed was considerably impaired by the plates being placed horizontally, which must have favoured the efflux of water from the cards, and thence between the surfaces of the zinc and silver. In this way he endeavours to account for the absence of the shock, as well as of other electric phenomena, of which the Colonel speaks, observing that the upright pile is certainly much stronger than a pile which has been immersed in water, and afterwards wiped.

The Colonel having given it as his opinion, that the effects of the pile were not augmented by its connexion with the conductor of an electrical machine; and likewise that the charging of a Leyden phial was retarded, when the apparatus was placed between its outside and inside coatings: Mr. NICHOLSON observes, that in the instance in which the pile was connected with the conductor, the electric stream of the latter may have been in a contrary direction to that of the pile. In the experiment in which the pile was connected with the Leyden phial, it is clear that the phial could not be charged higher than the pile itself; and the general effects of galvanism lead to a conjecture, that the galvanic apparatus resembles a large surface of an electric, charged to a low intensity.

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* Philosophical Journal, vol. iv. p. 242.

Colonel HALDANE afterwards made a series of experiments, with a view to the ascertainment of the relative powers of the different combinations of metallic substances. He found that the pile acted more powerfully when it was immersed in a given quantity of oxygen gas, than when it was confined in the same bulk of atmospherical air. In azotic gas, and in a vacuum, it was entirely suspended. From the experiments which led to these results, and from an inquiry into the state to which atmospherical air is reduced in a glass receiver, placed over a galvanic apparatus, and confined by water, he was led to coincide in opinion with FABRONI, that the galvanic effects depend on a chemical operation, and are for the greater part produced by the attraction of oxygen from the atmosphere. Consequently, he observes, according to the present theory, the whole of the operation must be considered as a combustion similar to that which arises from the combination of sulphur and iron filings with water.

The following experiments, made by Dr.

Henry Moyes, of Pittenweem, Fifeshire, North Britain, were communicated by him to Dr. Garthshore*. The interest they cannot fail to excite, induces us to give them in the author's own words, without attempting an abridgement.

"*Experiment* I. When 72 square pieces of copper, 72 of zinc, and 71 of moistened pasteboard, each containing four square inches, and not exceeding in thickness one-sixteenth of an inch, were formed into a column, by being applied to each other in the galvanic series, viz. zinc, copper, pasteboard—zinc, copper, pasteboard, &c. &c., the column, though entirely composed of oxydable materials, discovered a strong galvanic power. When the bottom of the column was touched with one hand, and the top at the same time with the other, both hands being wet with brine, a shock was felt fully as strong as that which was given by an electrical jar having 80 square inches of coated surface. Hence it appears that silver, or a non-oxydable metal, is by no means essential to the excitation of the galvanic power; and indeed I apprehend that a galvanic column, consisting of copper, zinc, and pasteboard, will be found as powerful as an equal column consisting of silver, zinc, and pasteboard. The above column, consisting of copper, zinc, and pasteboard, retained its power with little diminution during a period of 36 hours.

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* Philosophical Magazine, vol. vii. p. 347.

" *Experiment* II. When pieces of copper, zinc, and moistened alumine were formed into a column, by being applied to each other in the galvanic series, viz. zinc, copper, alumine, &c. the column gave shocks remarkably stronger than those it would have given had pasteboard been used. Hence a series of curious experiments spontaneously present themselves, demanding to be tried. Hence a probability that the galvanic power may sometimes occur among the strata of the earth; and hence a new theory of earthquakes will in all probability ere long appear.

" *Experiment* III. When the alumine in the galvanic column was moistened with brine, instead of water, the surface of the column became, in the course of three or four days, covered with a copious white efflorescence, which, when collected and examined, proved to be soda nearly saturated with carbonic acid. A like efflorescence was found in a column, in the construction of which no alumine was used. The column consisted of copper and zinc, stratified with paper moistened with brine, and furnished, you perceive, a pleasing probability that the galvanic action of the metallic substances may one day conduce to the arts of utility; it being capable of furnishing, when duly employed, genuine soda from the salt of the sea.

" *Experiment* IV. When the galvanic influence was transmitted, by means of two brass wires, through an aqueous solution of the carbonate of potash, both were corroded where they touched the solution, and both produced or extricated gas. The wire, however, which came from the copper, was manifestly less corroded than the other, though it yielded the largest proportion of gas; and the phenomena were not perceptibly changed by substituting lime in place of the alkali.

" Both the wires also gave gas when they were properly immersed in some other fluids; but when inserted in the legs of an inverted glass syphon, which had been previously filled with distilled water, the mode of their action was seemingly changed: one was corroded without giving gas, whilst the other gave gas without being corroded; and these phenomena were not interrupted by filling the bend of the syphon with mercury. One of the wires was constantly corroded without giving gas, and the other gave gas without being corroded, whether the interval between their extremities was filled with pure water alone, or partly with water and partly with mercury. The mercury, however, must have wholly precluded the transmission of oxygen from wire to wire. It seems impossible for oxygen, in any condition, first to descend and then to ascend through a column of a fluid full ten thousand times heavier than the air at the level of the ocean. Nothing, except the galvanic influence, appears to have passed from wire to wire, either without or within the syphon; and, if this be admitted, it will scarcely be affirmed that the corrosion of the wire was a simple oxydation.

"The galvanic action of various fluids, upon the whole or most of the oxydable metals, has lately opened a field of research, which seems well entitled to persisting attention. I have already projected a set of experiments, which I trust will afford it some degree of light. You shall have the results as soon as they are obtained, provided they shall seem deserving your notice; and I shall now conclude with briefly subjoining, that when a metallic communication was formed in the dark between the ends of a powerful galvanic column, an appearance of light, resembling a small electric spark, was distinctly perceived at the moment of contact: that the copper and zinc, when applied to each other, discover a strong galvanic power; yet plates of brass had no such power, even when piled with moistened alumine: that alumine appeared to be more galvanic when moistened with brine, than when moistened with water; that pasteboard was decidedly more galvanic when moistened with weak sulphuric acid, than when moistened either with water or brine; and that by inclosing a powerful galvanic column in a box, armed in the manner of a magnet, I have constructed a kind of galvanic machine, which is easily carried from place to place, which is extremely convenient in many experiments, and which has enabled me to explain the powers of the torpedo more clearly, I believe, than has hitherto been done.

Galvanic experiments, similar to those of Professor ALDINI, have been recently made by Mr. CARPUE, in the presence of Dr. PEARSON, and several other medical gentlemen, on the body of MICHAEL CARNEY, the criminal executed for murder. The principal object was to ascertain, whether galvanism, applied immediately to the nerves, could excite action in the internal parts, and particularly those subservient to respiration. With this view, an opening was first made into the windpipe, and about three pints of oxygen gas thrown into the lungs; the phrenic nerve was then exposed to conductors applied to it, and to the inside of the rectum, the lungs being at the same time occasionally inflated; yet no action could be excited in the diaphragm:—the nerves do not seem to be conductors of the galvanic fluid. Conductors applied to the inside of the rectum and nostrils, excited very considerable contractions in the right auricle more than three hours after death ; the ventricles were, as in Professor Aldini's experiments, perfectly motionless ; the distortions of the muscles of the countenance, &c. were nearly the same as on the former occasion.—The experiments were conducted with perfect accuracy and science ; but no new fact appears to have been ascertained.

CHAP. XVI.

Review of the researches of the different Continental Philosophers, after the discovery of the Voltaic pile.

IN the month of October, 1801, M. VOLTA addressed a letter to the celebrated Professor VAN MARUM, to request of him to make, in concert with Professor PFAFF, of Kiel, several experiments on a large scale, with the well known apparatus belonging to the Teylerian Museum at Harlaem, on the electricity of the metallic pile. The following is a faithful analysis of the joint labours of these two naturalists, who appear to have confirmed the doctrine of VOLTA, relative to the identity of the galvanic and electric fluids. Their researches are deserving of a particular notice, insomuch as the experiments on which they are founded, were made with the utmost care, and the most scrupulous attention. Their results must consequently tend to throw a great light on the theory of galvanism.

Notwithstanding M. VAN MARUM was nearly convinced, both by what he had seen, a few months before, of the effects of a large column, and by the experiments communicated to him by Professor Volta, that the action of the pile is purely electrical, still the theory of these effects, which he had himself adopted, differed in several points from that of the latter naturalist. Being desirous that the apparatus under his charge should be made to contribute, as much as possible, to the progress of the sciences, he solicited M. Pfaff to examine, with him, the two theories, in each of their different relations, by the means of the above apparatus, and by experiments made on the largest practicable scale. In the execution of this task they employed ten days, having been obliged to have several new machines constructed.

In his communication to Professor Volta, M. Van Marum begins by a detail of the results of the experiment which the latter had particularly pointed out to him, namely, of the one in which a very large battery was to be charged by a very short contact of the column. He employed a part of the large Teylerian battery, consisting of one hundred glasses, each of which contains five square feet and a half of coated surface, and has, in general, pretty nearly a line of thickness. The variations in the thickness of the glasses were productive of a change in the charge of each of them. Our naturalists having made choice of twenty-five glasses, each of which, when tried separately, was perfectly well charged by the column, set aside, in the first instance, four of them, for the composition of a battery, which they charged by the means of a full column, consisting of two hundred pairs of metallic plates of silver and zinc. On examination, they found that a single contact, as short as possible, sufficed to charge the battery to a tension equal to that of the column, which produced a divergence of five-eighths of an inch, of the gold leaves of BENNET's electrometer. Having augmented the battery to twenty-five glasses, which contained altogether 137 square feet and a half of coated surface, it was charged by a single contact, as short as possible, of the column, exactly to the same tension which had produced, in the gold leaves of BENNET's electrometer, a divergence of five-eighths of an inch. This decisive experiment having been made, our naturalists had little doubt, but that, by the mere contact of the column, they would have been enabled to charge, in the same way, the entire Teylerian battery, which contains 550 square feet of coated surface. They were prevented, by particular circumstances, from making this experiment, which they purposed to try in a more convenient season, when it would be unnecessary to take any precautions against the humidity of the atmosphere.

The pile, or column, with which they charged the battery containing 137 square feet and a half of coated surface, having, in the pairs of metallic plates, the silver below, and the zinc uppermost, consequently had the positive electricity above, and transmitted it to the internal surface of the battery, seeing that our naturalists made the contact of the column above. Having reversed it, in such a way as that the silver belonging to the metallic plates was above, and the zinc below; and having, with this inverted column, repeated the same experiments at its different heights, as well as with the totality of its parts, they observed that the battery was constantly charged, by a single contact, to a degree of tension equal to that of the column.

They afterwards tried the shocks or sensations produced by the discharge of the battery, charged at different heights of the column, and for this purpose employed two conductors of copper, each of them having a diameter of two inches, which conductors they held in their moistened hands. On beginning by the discharge of the battery, charged to the twentieth part of the column, they felt very distinctly the passage of the current, from the conductor into the hand, and from the hand into the conductor. One of the friends of M. Van Marum felt it as high as the wrists. Having, in a similar manner, continued to experience the sensations and shocks of the discharges of the battery, charged by forty, sixty, eighty, a hundred, a hundred and twenty, a hundred and forty, a hundred and sixty, a hundred and eighty, and two hundred pairs of metallic plates, the effect of the discharge on the body augmented, in proportion as the battery was charged by a greater number of the plates. When it was charged by the entire column of two hundred pairs, the commotions, which were very powerful, were felt as high as the shoulders; but those of the battery were not equally forcible with those of the pile by which it was charged.

M. Van Marum enters here into the details of an experiment, which required very particular precautions. It was made with a view to ascertain whether the contact of the conductor gave out a greater portion of electricity to the battery, than the action of the Teylerian electrical machine, having a plate of thirty-one inches diameter, would supply during the same contact. On repeating this experiment several times, he found that six contacts of the conductor charged the battery to the same degree of tension as a single contact of the pile. From hence it follows:

1st, That a Voltaic pile provided with two hundred pairs of metallic plates, furnishes, to charge a battery to the tension above pointed out, three times as much of the electric fluid, as the great machine could have supplied, in the primitive state in which it remained from the year 1785 to 1789.

And, 2dly, That the force of the above pile, in charging great batteries, and that which the great Teylerian machine possesses at this time, to charge them to an equal degree of tension, are in the ratio of three to five.

Relatively to the comparison of the sensations, or shocks, communicated by the discharge of the battery, charged, to different degrees of tension, by the contacts of the conductor of the electrical machine, with those of charges of the same tension by the means of the pile, he was convinced, by repeated experiments, that there is no perceptible difference in the shocks communicated by the discharges of the battery, whether charged by the machine, or by the pile, provided the tensions of the charge are equal. He flatters himself that he has proved, in the most decisive manner, by an experiment on a large scale, the identity of the current of the fluid put in motion by the Voltaic pile, and of that to which an impulsion is given by an electrical machine. The identity of the currents of each apparatus having been thus clearly established, and placed beyond any doubt, it follows as a direct consequence of the above cited experiments, that there can no longer be any question of *the action of a particular fluid*, which had been inconsiderately supposed to reside in the Voltaic column.

These experiments, added to those made by Professor VOLTA himself, besides prove, that the action of the above column, or pile, produced by the contact of two metals which touch each other, can be no other than an electrical effect. As the results they furnish may be explained by the action of two diverse metals, or of two different substances which are employed, the supposition of a particular fluid seems to be entirely done away; and, consequently, the true denomination of the Voltaic pile, ought in future to be *electric*, and not *galvanic*.

This furnishes a reply to the question which was proposed in the month of May 1801, by the Society of Sciences of Harlaem, conceived in the following terms: Can the Voltaic pile be explained in a satisfactory manner, by the known laws and properties of electricity; or is it necessary to conclude the existence of a particular fluid, distinct from the one which is denominated electrical? M. VAN MARUM, as well as his cooperator, M. PFAFF, had been before nearly persuaded of the existence of a particular fluid, connected with the phenomena of the pile, because they could not, without such a supposition, fully explain to themselves the results of the experiments made by several naturalists, and, among others, of a particular experiment tried by FOURCROY, VAUQUELIN, and THENARD, on which we shall touch in the sequel of the present chapter.

Another very singular and surprizing result, which was afforded by the experiments of our naturalists, conducted on a large scale, and which, according to them, was most decisively proved, is, that the current put in motion by the Voltaic pile, has an enormous celerity, which surpasses all that the imagination can conceive. This was particularly demonstrated by the following experiment: A battery, having one hundred and thirty-seven square feet and a half of coated surface, was charged by the pile, to a tension equal to the one possessed by itself, by a contact, of as short a duration as possible, of the communicating wire. This contact did not last for the twentieth part of a second. When we reflect on this experiment, it will not appear surprizing that the apparatus of VOLTA should produce effects, such as have never been seen when common electrical machines have been employed. The rapid decomposition of water may be particularly cited on this head. The above phenomenon induced M. VAN MARUM to endeavour to augment the force of the Voltaic pile, so as to give it a still more decided superiority. The efforts he made to accomplish this, consisted, in the first instance, of a more perfect insulation of the pile. It was proved by this insulation, that the shock produced by a pile formed of two hundred pairs of plates, has so violent an effect on the body of the person by whom it is received, that those by whom it has been once felt, cannot prevail on themselves to receive it a second time.

M. Van Marum afterwards changed the pile, or, in other words, caused one to be constructed with one hundred and ten pairs of metallic plates of very large dimensions, composed of copper and zinc. With this pile he made experiments, similar to those which had been tried by other naturalists, on the fusion of iron wires. He was convinced, by one of these experiments, of the impossibility of constructing a pile of a considerable height, with large plates which shall be productive of an effect proportioned to their number, seeing that their weight occasions too great a pressure on the bits of pasteboard at the inferior part of the pile. The considerable thickness of the plates in question is the cause of this inconvenience. He therefore contrived to divide the pile into several, in such a way as to form with them a kind of chain, in which the combination of the different columns would be managed without difficulty, and with which it would be easier and more convenient to make the experiments, than with a pile of a considerable height. Having disposed these piles, four in number, and containing collectively one hundred and ten pairs of plates, in such a way as to establish a communication between them, the effects which resulted from them were as follows:

1st, The two piles which contained together fifty pairs of copper and zinc plates, produced a strong red heat in eight inches of iron wire, which was partly melted.

2dly, The two piles containing together sixty pairs of plates, produced a red heat in six inches of iron wire. This smaller and disproportionate effect our naturalist ascribed to the pieces of pasteboard not having been properly moistened.

3dly, The four piles, when united, produced a red heat in twelve inches of iron wire of the same thickness.

4thly, The wire is not perfectly fused, unless when it touches, in an immediate way, the edge of the upper plate of the column.

5thly, The result of particular experiments, made with a view to ascertain whether there is a very sensible difference between the sparks which are emitted, and those which enter, was, that neither M. Pfaff, nor M. Van Marum, nor any of the friends who were present at the experiments, could observe the smallest difference between the emanative sparks (the positive), and the entering sparks (the negative), in the conducting wire. They simply noticed equal irradiating sparks.

6thly, The latter experiments enabled our naturalists to observe a very fine phenomenon, well calculated to attract the attention, more particularly of those who, being present at philosophical experiments, are fond of witnessing the splendid effects they occasionally produce. When they touched the surface of the mercury with the extremity of a very thin iron wire, a combustion of that part of the wire ensued. The process was carried on so forcibly, that sparks were dispersed on every side, forming thousands of apparent rays, which represented fine suns of a considerable diameter. This beautiful spectacle they were enabled to continue at pleasure, by gently depressing the extremity of the iron wire, in proportion as the metallic particles were dispersed by the combustion.

7thly, When they employed wires, which were too thick to be melted, the oxydation of the mercury, on its surface, was rendered more visible by each spark. It formed spots of more than a line in diameter.

8thly, The extremity of a wire of platina, of about $\frac{1}{73}$ of an inch in diameter, was melted, and formed a globe.

9thly, The sparks, more particularly those from the extremity of the communicating wire, when it was not too thin, had a diameter of more than the tenth of an inch.

Having repeated these experiments, which demonstrate the great powers of the pile he had contrived, M. Van Marum proceeded to examine the tensions, the shocks, and the charges it was capable of communicating to the battery. One of the most essential points, according to him, to be enabled to obtain, from a large pile, the greatest possible effect, and on which the successful result of the experiments in a great measure depends, consists in the proper degree of the humidity of the pieces of pasteboard, or of the other substances which are interposed between the metallic pairs of plates; seeing that too great a proportion of moisture is equally injurious with its defective state. A saturated and cold solution of the muriate of ammonia is best calculated for these experiments. M. Pfaff was convinced, as well as M. Van Marum, that a warm saturated solution, notwithstanding it may contain a greater proportion of the muriate of ammonia, has not an equally powerful effect. Other circumstances, which had been hitherto unnoticed, and which M. Van Marum proposes to investigate hereafter, have a powerful influence on the effects of a large pile, which are thus exposed to greater or less variations. He made several researches relative to the causes of the more considerable effects of large piles, in the fusion and oxydation of metals.

The results of these inquiries are, that the tensions of two piles, having an equal number of plates, although of very different diameters, are absolutely the same; and that a battery is equally well charged by either of them. To conceive how two piles, the tensions of which are so similar, and which are equally efficacious in charging large batteries, should have such different effects, relatively to the fusion of metals, it is essentially necessary to distinguish the action of an insulated pile, from that of a non-insulated pile, by the current, which is different in the one and in the other, as well as by its greater degree of celerity, and by the quantity of the fluid which the discharges transmit in equal times.

M. Van Marum next examines what are the circumstances, or the causes, which admit or produce a greater celerity, in the current propagated by a pile of the largest dimensions. On this subject he made several experiments, which seem to prove that one of the principal causes of this effect, is, that the humid substances placed between the metallic plates, and which ought not to be considered as the best conductors, present fewer obstacles in a large column, on account of their greater breadth. In this opinion M. VOLTA perfectly coincides with him.

He proceeds to communicate to the latter a variety of experiments made by him, principally with a view to explain whence it arises that the piles have a more sensible effect, when the humid substances contain muriate of ammonia, and why the current is then much more powerful. A large pile of twenty pairs of plates, constructed with pieces of pasteboard dipped in the muriate of ammonia, melted into globules, in the usual way, four inches of iron wire; while another pile, of the same size, and containing likewise twenty pairs of plates, provided with pieces of pasteboard dipped in a solution of muriate of soda (common salt), neither melted, nor even ignited, a single line of iron wire. A third pile, constructed with pieces of pasteboard dipped in pure water, produced a still smaller effect, relatively to the sparks. There can, therefore, be no longer any doubt, but that the quantity of the electric fluid, in the current of the first pile, was much greater, or, which is the same thing, that this current had a much greater celerity, than that of the columns in the case of which the pieces of pasteboard were either dipped in water, or in the solution of the muriate of soda.

But what is the cause of this greater celerity in the motion of the current? Is it to be ascribed to the circumstance of the muriate of ammonia oxydating the metals after its decomposition? This decomposition is fully proved in the column, by the strong ammoniacal smell which is perceived shortly after it has been constructed, and, still more particularly, after it has been taken to pieces. It is likewise evidently to be perceived, that the muriatic acid of this decomposed salt, strongly oxydates the surfaces of the copper and zinc; and that this oxydation, in proportion as it becomes less, weakens also, in the same ratio, the effect of the column.

The above experiments appeared to M. Van Marum to be highly favourable to the opinion, that the oxydation of metallic substances, considered as such, contributes powerfully to augment the effects of the pile. As he still, however, entertained doubts relative to the explanation of this phenomenon, at the same time that he was not certain but that some other unknown cause, or circumstance, might have combined itself with the oxydation, or with the source from whence it originated; and as, besides, M. Pfaff had not, relatively to the oxydation, considered as such, the same ideas with himself, he formed the resolution to make, in concurrence with the latter, a series of experiments which should illustrate this fact, and remove every doubt which could possibly be entertained. The following is a concise detail of these experiments, and their results.

Experiment I. Our naturalists steeped bits of pasteboard in the sulphuric, nitric, and muriatic acids, sometimes concentrated, and at other times diluted in a greater or less degree, employing a pile of thirty pairs of metallic plates, of zinc and copper, each of them having a diameter of an inch and a half. They likewise employed piles constructed with ten pairs of plates having a diameter of five inches. They were desirous to ascertain whether the effect of these different piles would be more or less powerful, in proportion as the oxydation of one, or of both the metals, should be produced with a greater degree of promptitude. They could not observe any augmentation, either of the tensions, of the commotions, or of the sparks, in proportion as the oxydation of the metals was more speedily brought about. These effects were even weaker in the experiments with the nitric acid, whether concentrated, or diluted, and in the case of which the oxydation of the two metals went on with great rapidity, than in a similar, comparative, pile, the pieces of cloth, or of pasteboard, introduced into which, had been dipped in a solution of the muriate of ammonia. It results from hence, that this experiment was by no means favourable to the opinion, that the oxydation of the metals, considered as such, is the principal cause of the augmentation of the effect of the current of the piles, in which the muriate of ammonia is employed.

Had this conclusion, however, been drawn in an absolute way, it would have been too precipitately made, seeing that it is possible, in the case in question, that the oxydation is brought about in a manner which differs from that of the acids, and that it is better calculated to produce the effect of the augmentation of the current of the pile. May not the action of the ammonia on the copper, likewise contribute, in a certain degree, to this effect? With a view to ascertain what this action can effect, alone, and without being combined with the oxydation produced by the muriatic acid of the decomposed muriate of ammonia, our naturalists dipped in ammonia, the pieces of pasteboard belonging to a column of thirty pairs of copper and zinc plates, each having a diameter of an inch and a half. They now observed, that the effects of the current of this pile were much weaker than those of a similar pile, the pieces of pasteboard belonging to which had been moistened with the muriate of ammonia.

Experiment II. With respect to the influence of the oxydation on the effect of the pile, they deemed it essential to inquire into the results the latter would afford, *in vacuo*, in different gases destitute of oxygen, and in oxygen gas, pure and without mixture. In this experiment, they employed a pile of sixty pairs of plates, of silver and zinc, having a diameter of an inch and a half, with pieces of cloth dipped in a solution of ammonia. The pile was placed beneath a cylinder covered over. After having nicely observed its tensions, and the effects of its current, that is, the sparks, the shocks, and the decomposition of the water, they made a vacuum in the cylinder. The above effects had now so great a resemblance to those observed in the atmospherical air, that no sensible difference was to be noticed. It may not be amiss to remark here, that the French naturalists, BIOT and CUVIER, had already proved, by similar experiments, that the action of the pile, which, when the process is carried on in the open air, absorbs a part of its oxygen, likewise takes place *in vacuo*.

The effects were likewise the same, after the pile had been *in vacuo* for a considerable space of time. The introduction of the carbonated hydrogenous gas, drawn from olive oil, and carefully preserved, was not productive of the smallest variation in the results. It was the same with the azotic gas, when introduced. But when the oxygen gas, in a very pure state, and drawn from manganese, was employed, the shocks produced by the pile were more powerful, and the sparks, larger, more brilliant, and obtained with greater facility. The vacuum having been made in the cylinder, the spectators were very much surprized to see that these effects were greatly diminished. The oxygen gas having been introduced a second time, the results were manifested in the same way as before. This experiment having been repeated a third time, in each of the different ways, the effects which were produced were perfectly similar. The variations in the results, relatively to the influence of the vacuum on the action of the pile, in the employment of the oxygen gas, occasioned a considerable degree of surprize in M. VAN MARUM. Before he should venture to come to any decision on this subject, it was his intention to observe, likewise, the impression of the azotic and carbonated hydrogen gases on the pile, after its having been exposed to their action for a considerable length of time; and to repeat this experiment in *vacuo*, and in the different gases, with piles the pieces of pasteboard introduced into which should be steeped in water, instead of the muriate of ammonia; seeing that the action of such a pile would be more uniform.

Experiment III. Finally, to judge of the influence of the oxydation on the action of a pile, our naturalists considered that it would be expedient to make a trial with piles, the pieces of pasteboard introduced into which should be steeped in solutions, not having, in the smallest degree, a tendency to oxydate the metals. Accordingly, the circular pieces of pasteboard introduced between twelve pairs of metallic plates, having a diameter of five inches, were steeped in a solution of potash, as highly concentrated as possible. This pile had a much greater effect than another of a similar kind, the pieces of pasteboard employed in which had been steeped in pure water. The sparks, which had a radiated appearance, were visible at the third pair of plates from below, and, above, at the twelfth pair, of the former of these piles; while, in the latter, they were scarcely perceptible at the eleventh and twelfth pairs. In the case of the former, when it was taken in pieces, the polished surfaces of the zinc and copper had not suffered the smallest change from the potash. The effect of circular pieces of pasteboard, steeped in the sulphuret of liquid potash, was much weaker, and of a very short duration.

In concluding his letter, of which we have made a very faithful analysis, M. Van Marum likewise communicates the results of several experiments made by him, at a lecture he had occasion to give in the Teylerian Hall at Harlaem, on the electric pile of Volta. In the first of these experiments, made with a battery, the number of the pairs of plates, having a diameter of five inches, employed in which, was, for the first time, augmented to two hundred, comprised in a chain of six piles, he succeeded in melting completely, into globules, an iron wire twenty-eight inches in length; and in igniting, throughout its whole extent, another iron wire thirty-eight inches in length. In the second experiment, several of the persons who were present, could not observe the smallest difference, after having successively felt the shocks of two piles, one composed of twenty pairs of plates of copper and zinc, having a diameter of an inch and a half only, and the other provided with an equal number of pairs of plates, of five inches diameter. The result of the third experiment was the same, relatively to the commotions of a pile constructed with twenty pairs of metallic plates having a diameter of ten inches; and of another pile provided with an equal number of pairs of plates, with a diameter of an inch and a half only.

The fourth experiment demonstrated that the force of a pile does not augment in proportion to the surfaces of the pairs of metallic plates, seeing that, in a pile, twenty pairs of these plates, having a diameter of five inches only, were capable of melting four inches of iron wire.

Such is the substance of the very interesting communication of M. VAN MARUM, on the identity of the electric and galvanic fluids. He announces, in a postscript, his intention to address another letter to M. VOLTA, containing a series of new experiments made by him.

In another communication made to M. BERTHOLLET, in Paris, this celebrated naturalist writes as follows: "Since my last, I have succeeded in the decomposition of water, by the means of the current of the electrical machine, provided with a plate of thirty-one inches diameter, constructed by me on a new plan*. As you requested of M. PFAFF to propose this experiment to me, I cannot do less than inform you of the mode I pursued in accomplishing the aim I had in view. I took a thermometrical tube, of the kind employed in making the most sensible thermometers of CRAWORD and HUNTER, for which purpose I had procured several of these tubes some time before in London. Its diameter interiorly, was not more than the hundredth part

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[*] This machine is particularly described in the *Journal de Physique* for June, 1795.

of an inch; and I introduced into it, an iron wire of the diameter of about the three hundredth part of an inch, to the depth of about twelve inches. I now closed the end of my thermometrical tube with sealing wax, in such a way as that the extremity of the iron wire should scarcely project; and placed the tube itself, by the means of a cork, within a larger tube containing water. The rest of the apparatus was arranged in the customary manner. By directing the powerful current of the above-mentioned machine to this apparatus, the copper ball belonging to which, placed on the thermometrical tube, was at the distance of about three or four lines from the conductor, I succeeded in decomposing the water with a promptitude nearly equal to that which results from a Voltaic pile of a hundred pairs of metallic plates."

In the month of April, 1801, several interesting galvanic experiments were made at Berlin, by Major HELVIGE, of the Swedish service, Professors BOURGUET and HERMANN, and Dr. GRAPENGIESSER. They began by the construction of a galvanic pile, according to the principles laid down by VOLTA, in the formation of which they employed gold and zinc. The former of these metals consisted of the Prussian coins denominated *double Fredericks*, not struck, and between these coins and circular pieces of zinc of the same size, bits of cloth moistened with salt water were introduced. By this apparatus the shocks, the sparks, and the decomposition of water into its two constituent gases, were readily produced.

They now constructed another apparatus, the piles belonging to which were composed of pieces of silver and zinc. They were desirous to ascertain which of the two modes would constantly produce the greatest effect; but as each apparatus separately afforded a great irregularity in its products, they could see no just motive for giving an exclusive preference to the one over the other. A fact, however, which had not been noticed, was, that when the effects of the two piles were combined, by the means of a metallic chain, fitted to the base of one of them, and to the top of the other, so as to be in contact, on the one hand, with the gold, and, on the other, with the zinc, so great an abundance of the galvanic fluid was produced, as to be capable of extending the shock to the shoulders; while, with the common apparatus, it was barely sufficient to render it sensible at the elbows.

Our naturalists found that the spark is to be obtained much quicker, either between the two conductors of a single pile, or between those of a combined system, when their extremities are terminated, one of them by the point of a bit of very small iron wire, and the other by a button.

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The spark, which, on the supposition that the extremities are not terminated in this way, is procured with some difficulty, soon appears under the above circumstances, and is accompanied by a powerful detonation. If it be intended, however, that this effect should have the greatest possible intensity, it is necessary to cover with gold leaf both the point and the button of the conductors.

This last discovery was the result of accident. It became necessary to consult BENNET's electrometer, to ascertain the effect which would be produced on it by the Voltaic apparatus. For this purpose the button was laid on the top of the electrometer, and the point introduced as far as the gold leaves, which instantly performed the function of a conductor, and determined, in a manner equally powerful and unexpected, the escape of the spark; a result which had been but imperfectly obtained by the common means. From that moment, the extremity of each of the conductors was coated with gold leaf, and a strong spark constantly obtained with promptitude and ease. This expedient was resorted to, with the utmost success, in inflaming phosphorus, sublimed sulphur, the fulminating gas, and, lastly, gunpowder, the combustion of which usually resists the action of the electric fluid. The following remark was, notwithstanding, made, that, in the course of these experiments, the gold leaf by which the extremity of each of the conductors was covered, was itself melted, and reduced into a globule, more or less perfect, according to the force of the spark.

Our naturalists ascertained, with Mr. NICHOLSON, the inverse state of the two conductors. When subjected to the test of the electrometer of condensation, the silver was found to be negative, and the zinc positive. They did not stop here; they charged a Leyden phial with the product of the pile, by the intervention of VOLTA's condenser. The two surfaces of the phial, which seemed to be obedient to the general law, were differently effected; and, which appeared surprizing, it did not afford a shock that could in any way be compared to the slightest simple galvanic shock, whatever precautions were taken in charging it; with whatever quantity of the fluid it was charged; and however this quantity was accumulated; notwithstanding a small fraction of such a quantity, proceeding from the Voltaic apparatus, would have produced a pretty violent shock.

Finally, being desirous to subject other electric effects to the test of galvanism, they succeeded in producing, by the help of two condensers, the figures of LICHTENBERG, with the same dissimilarities, and with forms resembling those which they assume in the electric fluid.

Such are, in substance, the results of the operations of the Berlin naturalists. It is to be lamented that they did not come to a certain conclusion, relatively to the nature of the fluid which is manifested in galvanic experiments, to the end that every doubt might at length be removed on that head. It is, unquestionably, impossible not to recognize the similitude between this fluid and electricity; but if any one were disposed to believe that there exists, between these fluids, a perfect identity, the experiments of the Berlin naturalists would be subversive of every presumption of that kind.

The last experiment made by them, and which is the one best calculated to induce a belief that galvanism and electricity are of a different nature, consists in their having insulated the apparatus, and, together with it, a glass tube, in which the galvanic influence produced, by the approximation of two conductors, a decomposition of water. They now went over the whole of this process with a pretty large electrical machine, the action of which they substituted to that of the Voltaic pile. Notwithstanding they, in this instance, obtained very strong sparks, the water contained in the tube did not undergo any change or modification. This would seem to demonstrate, not merely that there is not any analogy between the two fluids, but likewise that their action is independent, and that the greatest abundance of electricity does not in any degree influence the galvanic process.

On another hand, if particular circumstances be well weighed and considered, such, for instance, as the more powerful action of the pile, produced by the means of the interposed pieces of cloth, dipped in a fluid charged with ammonia, by which the oxygen is readily disengaged: if we next consider that, by the interposition of an animal substance between the metallic plates, it becomes apparent that the flesh is irritated and reddened, on the side which is in contact with the silver, while it is reduced into a greasy, or de-oxydated animal substance, on the side which touches the zinc: if we consider likewise, that the apparatus is so much the more susceptible of energy, as it is better provided with metals subjected to the action of the oxygen; and, finally, that the metallic plates are sensibly corroded, after the pile has been made to act for any length of time; and that, when there is no longer any oxygenous substance interposed between the metals, the galvanic action ceases entirely; it will hence be impossible not to avow that the presence of oxygen, and likewise its action on any given substance, are necessary conditions to the production of galvanism; and that the galvanic phenomena, not only have a cause extraneous to that of electricity, but likewise have an absolute dependence on the oxygen, without the concurrence and action of which it has been hitherto impracticable to produce the effects of what is denominated the *galvanic fluid.*

The galvanic experiments of Professor TROMSDORFF, of Erfurth, are well entitled to notice. He constructed a pile of a hundred and eighty layers of copper, zinc, and pasteboard, with which he obtained violent shock, and very strong sparks. After having, by the means of this apparatus, brought about the disengagement of oxygen and hydrogen gases, as well as the oxydation and de-oxydation of metals, he subjected to its action different metallic substances, and, among others, the metals which are not oxydable in air; the whole of which were ignited by the galvanic process. A bit of fine gold leaf, when made to communicate with the extremity of the pile which was terminated by the zinc, burned, decrepitated, and produced a vivid light. A bit of silver leaf burned with a blue flame; brass, with a reddish blue flame; copper, with a blue flame inclining to the colour of the emerald; zinc, with a whitish blue flame; tin, with a reddish white flame, &c.

For this purpose it is necessary that the metals should be finely laminated. The Professor did not entertain any doubt, but that, by augmenting the force of the apparatus, metallic substances might be burned in still more considerable masses. When he fell on the expedient of oxydating the metals stiled *noble*, in hollow spheres of glass, they were entirely consumed, and distributed themselves over the internal surface of the globes.

He expresses himself in the following manner, on the subject of the resolution of water into oxygen and hydrogen gas, by the galvanic fluid. "We are at this time fully occupied in Germany with galvanic experiments. M. Ritter, a young man of very conspicuous talents, devotes himself entirely to this branch of natural philosophy, relatively to which he has made several very ingenious experiments. He flatters himself that he can prove, in a clear and satisfactory manner, that water is a simple body; and his friend, Professor Pfaff, asserts that he has transformed this liquid into a corresponding quantity, either of oxygen gas, or of hydrogen gas. My pile is extremely powerful, and consists of plates, or discs, of zinc and copper, with round pieces of pasteboard impregnated with salt water. It is of great importance that the pile should be well insulated, and, more particularly, that the fluid should not flow, in the smallest proportion, on the edges of the discs. Notwithstanding I cannot account, in a satisfactory manner, for the effects of the Voltaic pile, I am still far from concluding, with RITTER and PFAFF, that water is an indecomposed body, and that the destruction of the splendid edifice of modern chemistry is the inevitable consequence of their experiments."

BRUGNATELLI, in his Annals of Chemistry *, has published a memoir under the head of "Chemical Observations on the Electric Acid." This memoir may, at a first view, appear to have but an indirect connexion with galvanism; but if it be considered that, to be enabled to conclude definitively and with certitude, that the electric fluid and the galvanic fluid are identical, it is necessary to know the real nature of the former of these fluids, the utility of the subject in question, in the prosecution of galvanic inquiries, will be readily admitted.

"Naturalists," observes BRUGNATELLI, "have hitherto merely abandoned one erroneous hypothesis for another, in considering the nature of the electric fluid. Some have regarded it as identical with heat; while others have been led to consider it as a modified caloric. The disciples of STAHL ascribed to it the nature of their

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* Vol. xviii. p. 136.

phlogistic, or, at the least, supposed it to be a fluid abundantly provided with that principle. HENLEY conjectured it to be phlogistic, when in a state of repose, and fire, when in a state of activity. Among the moderns, several have been found who have declared it to be an acid; but their opinion has been combated by GARDANI, who, by the means of several ingenious observations, has endeavoured to demonstrate that it is composed of caloric and hydrogen."

The results of a great number of electrical experiments, recently made by BRUGNATELLI, have led him to subscribe to the opinion of the naturalists who consider it as a fluid differing from all others hitherto known. Entertaining no longer any doubt but that it is of an acid nature, he has classed it, in the system of chemical bodies, under the denomination of *oxi-electric* (electric acid). It is, he observes, a fluid nearly equal in subtility to the fluids of light and caloric. It has a peculiar and disagreeable smell, which has some resemblance to that of phosphorus. It has a pungent and acid taste. It irritates and enflames the skin. It is true, that this inflammation speedily subsides, when the part is washed with water in which a small proportion of ammonia has been dissolved. If the electric fluid be transmitted to a wound, it occasions a burning sensation, similar to the one which is produced by any other acid. It reddens the tincture of turnsol, which recovers its blue colour in proportion as the fluid passes off. It penetrates into metals, into the substance of which it insinuates itself, with a greater or less facility, according to their nature. When in its active state, it dissolves them in the same way as water dissolves salts, transporting their particles to long distances, through a great number of bodies. It is also soluble in water.

If, in an aqueous solution of the electric oxyd, metals be steeped, the greater part of them will be oxydated at the expence of the water, and hydrogen gas will be disengaged. But, with respect to the metallic oxyds, our naturalist has found that the presence of the electric acid renders them saline, and favours the productions of *electrats*. The electrat of copper is of a fine and transparent green colour; that of zinc of a deep and opaque grey colour; that of silver, of a transparent white; that of iron, of an opaque red, inclining to yellow, &c. The metallic electrats are not soluble in water; but the electric acid is capable of transporting them to considerable distances through that medium, and of thus depositing them on other metals of a different nature. They, in this case, assume the appearance of saline crusts, sometimes irregular, and displaying, at other times, a surprizing regularity.

The experiments which led our naturalist to ascertain the acid nature of the electric fluid, were publicly repeated a considerable number of times, in the presence of several accomplished chemists, among whom is to be reckoned his very learned colleague and friend VOLTA. The latter engaged to furnish him, for the subsequent volumes of his Annals of Chemistry, with several new inquiries on the same subject *.

The electrical, or, rather, galvanic apparatus, employed by our author in the above experiments, was the one which VOLTA, by whom it was invented, denominates *chapelet de tasses*, or, in other words, it consisted of a range of cups. He plunged into one of these cups a small square lamina of zinc, soldered to a curved length of brass wire, which was brought over into the adjacent cup, and thus consecutively. The cups, fifty in number, were all of them filled with water impregnated with salt. The metallic arcs were stationed in such a way as that the zinc should constantly precede the brass. The two cups at the extremities were made to communicate with each other by the means of a large metallic wire.

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* On this occasion he likewise promised to give an exact description of the apparatuses employed by him, several of which, having a peculiar effect, and a particular construction, are not as yet known,

The first six paragraphs of the chemical observations of BRUGNATELLI, contain a detail of experiments which could not be well comprehended without the plates by which they are accompanied; but which prove that, by the means of the electric acid, very beautiful crystallizations are to be obtained on different metals. In the fifth paragraph he observes, that he has seen crystals of silver formed on a rod of platina, plunged into the same cup with a silver wire. These crystals had been driven to the platina by the electric current. In another experiment he observed that the silver, when crystallized on the platina, preserved all its metallic brilliancy.

In the three following paragraphs our author explains the chemical characteristics of the electrats of silver, tin, and iron. On this subject he expresses himself as follows:

"The transparent crystals of silver, which appeared, either on that metal, or on any of those cited above, were occasionally irregular, and assumed, at other times, the most decided shape. They represented long flattened prisms, having hexagonal extremities with small surfaces, which powerfully refracted the light. They were insipid to the taste, and were not soluble in boiling water. When exposed to a strong heat, they shot out in efflorescences, and, having entirely lost their primitive configuration, resembled small flakes of snow. The air, more especially when dry and warm, deprived them in the same way of their water of crystallization. They were dissolved by the nitric acid, with a very strong effervescence, which was likewise produced when the acid was poured on the salt in an efflorescent state. The nitric solution was very limpid, and was decomposed by the alkalies, affording a precipitate soluble in all the acids, in the muriatic even."

Our author did not examine the gas which is driven from the electrat of silver by the different acids, and more particularly by the nitric and muriatic. He had reason to think, however, that it is not the carbonic acid gas, as may have been conjectured, having satisfied himself that his electrat is likewise formed on metals immersed in lime-water. He was of opinion that it must be a particular gas, a gas *sui generis*; unless, indeed, it should be found to be the electric acid itself. He forbore, however, to come to a decision on this head, in the expectation that VOLTA, who was at that time employed in examining the gases disengaged from different bodies by the electric acid, would likewise ascertain the nature of the one in question.

When, in these experiments, tin was employed instead of silver, our naturalist obtained saline crystals, similar to those which the latter of these metals had supplied: they formed a deposition on the other metals; were brilliant, transparent, soluble in the acids, in which they effervesced, and were precipitated in the form of a whitish powder. The crystals he obtained, when he employed brass and steel, were regular cubes: they were transparent, and soluble in the acids, in which they effervesced. The nitric solution afforded a blue precipitate with the prussiat of lime. This result rendered it doubtful whether the crystals obtained in these experiments were iron or silver, or rather a mixture of the two metals. To ascertain this, M. BRUGNATELLI made another experiment, which is followed by several observations.

To obtain the crystallized electrats, it is necessary that the metals should be well cleansed, and that they should not have been employed for any length of time in electric experiments. It is likewise essential that the electric current should be slow and progressive. VOLTA has observed, that a powerful apparatus has never supplied them with any degree of regularity. BRUGNATELLI likewise noticed that their formation was more speedy, and the form of their crystals more beautiful, when the apparatus was exposed to the direct rays of the sun.

The electric acid is not decomposed in oxydating the metals, but effects their oxydation at the expence of the water. It has this property in common with the other acids, which do not contain the basis of air unless in the oxygenous state, which, according to Brugnatelli, is incapable of oxydating or acidulating a metallic substance*, this process not being brought about, unless by the thermoxigen. The acids, however, which contain this basis in a more or less complete state of thermoxigen, such as the nitric acid, which, agreeably to the experiments of Brugnatelli, is composed of the oxyd of azote, of thermogen, and of oxydated muriatic acid, are well calculated to produce this effect. Several thermoxydations being, notwithstanding, produced without the disengagement of gas, our naturalist thinks it essential to point out that he ascribes them to another cause, which has not yet been ascertained.

He noticed, on several occasions, the great affinity of the electric acid with thermoxigen. It is well known that it decomposes the metallic thermoxyds, those even which it thermoxygenates. Dr. Priestley has observed, that the electric spark vitiates the air, by acting on that fluid as a phlogisticating substance. This depends on the combination into which the electric matter enters with the basis of the pure air. To a similar combination may be ascribed the formation of the nitric acid, during the passage of the spark through the nitrous gas. The explanation which naturalists had before given of this phenomenon was extremely unsatisfactory.

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* Arsenic, molybdena, cobalt, &c. unite with oxygen, after being thermoxygenated.

"Although," observes **BRUGNATELLI**, "the passage of metallic electrats, through metals, is still difficult to conceive, it is certain that the electric acid possesses a sufficient energy to attenuate these substances, and to break them into almost imperceptible parts, in such a way as to transport them through all the bodies which are permeable to itself, without the nature and quality of the metals being on that account changed." The difference of savour which is impressed on the tongue by two different metals, appears to our naturalist to depend on this singular combination of the electric acid with the pure metal. The metallic taste is extremely manifest in this experiment, more particularly when gold and zinc, silver and zinc, zinc and copper, and *vice versa*, are employed as exciting arcs.

Of all the metals, gold and platina alone appeared to **BRUGNATELLI** not to be sensibly affected by the electric acid. In the course of his experiments, made with the Voltaic apparatus of a range of cups, he frequently observed the silver detach itself from a conductor of that metal, and fix on the platina and gold, which were thus covered by a silvery surface. He likewise observed that mercury whitened gold, notwithstanding the latter was immersed in water, and at the distance of at least six lines from the mercury. In other experiments of a similar kind, in which conductors of gold and silver were immersed in the same cups with zinc and copper, the surfaces of the former of these metals were covered by the particles which detached themselves from the latter.

Having terminated our extract of the memoir of M. BRUGNATELLI, we proceed to the interesting report made to the French National Institute by Professor CUVIER, on galvanism, and on the experiments of three celebrated chemists, FOURCROY, VAUQUELIN, and THENARD. As it would be impossible to give an analysis of this report without depriving it of a great part of its merit, we give it at full length.

" Chance, the parent of almost every discovery, has within these few years afforded its aid to naturalists in such a way, as to render the present epoch a very remarkable one in the history of the sciences. A few pieces of metals, on being brought in contact, have displayed phenomena which no human sagacity could have foreseen, and have opened to us a field as capacious as it is fertile in its applications. The influence of these phenomena is now become more extensive than ever. They were, in the first instance, apparently confined to the animal economy, but appear at this time to have an important concern in the greater part of the operations of chemistry.

"We are more particularly indebted to the genius of VOLTA, for the new lights which have been thrown on this subject. His opinion, that galvanism was merely an application of chemistry to the animal economy, having been contested by several scientific men, he sought the means to augment its effects in such a way, as to render their true nature evident to all the world. He accordingly found that, by multiplying the pieces of two different metals, by constantly disposing them alternately, and by keeping them moist, he could produce attractions, repulsions, and shocks, entirely similar to those of the Leyden phial. He found, in short, that a pile of discs, alternately formed of silver, zinc, and moistened pieces of pasteboard, instantly manifests all the appearances of vitreous electricity at the extremity at which the silver is stationed, and of resinous electricity at that of the zinc. There is, however, this difference, that a Leyden phial, being once discharged, ceases to have any effect, unless it be again electrified; instead of which, the Voltaic pile electrifies itself, insomuch that its effects are incessantly renewed, and cannot be diminished for an instant, unless it be discharged by the means of very large conductors. In addition to this, the Leyden phial is constantly discharged by the presence of water: it is sufficient that there be the smallest portion of humidity between its two surfaces, that its effects should be destroyed. But it is in vain that the water with which the pieces of paper are moistened flows out from the pile; its effect still preserve their intensity; and do not cease until it is entirely plunged into an aqueous medium.

"These differences could not fail to inspire doubts of the perfect identity of galvanism and electricity; to augment which doubts, other phenomena, of a still more extraordinary nature, have intervened. If the extremities of two metallic wires, which communicate, one of them with the resinous, or negative extremity of the pile, and the other with the vitreous, or positive extremity, be plunged in water; and if they be held at a small distance from each other; at the extremity of the former of these wires, bubbles of hydrogen gas will be disengaged; and at the extremity of the other, oxygen, which fixes on the metal when the latter is oxydable, and rises in bubbles when it is not so. This action continues, so long as the apparatus remains in the state which has been described. It is not in this fact, however, that the greatest singularity of the phenomena consists; and it is here that galvanism begins to enter on the domain of chemistry.

“ It would have been quite natural to consider this gas as the product of the decomposition of matter, if a particular circumstance had not likewise thrown doubts on this explanation. It is necessary, to the end that the disengagement may take place, that the extremities of the wires should be at a certain distance from each other; since, if they were to touch, the bubbles would cease to be distinguishable. Why, then, do the oxygen and hydrogen, proceeding from the same particle of water, appear at two distant points? And why does each of them appear invariably on the wire which belongs to one of the two extremities of the pile, and never on the other?

“ Such was the state of our galvanic knowledge, when a report * was made to the Institute, of the proceedings of the class of natural philosophy, towards the close of the year 1801. All the experiments which had been made in the different countries of enlightened Europe, after they had been classed, and subjected to a rigid examination, by a commission, of which M.

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* See vol. i. of this work, p. 363.

Halle was the reporter, simply led to the three following great and principal results. In the first place, that the intensity of the fluid denominated galvanic, is augmented in proportion to the number and extent of the metallic surfaces brought in contact. Secondly, that its action is perpetually renewed. And lastly, that gases are produced by the communication of the two extremities of the pile with water.

"Since that time, however, naturalists have displayed an uncommon assiduity in the efforts they have made. Their curiosity has been particularly attracted by the last of the above phenomena. Some of them have fancied they perceived in it the bases of a new chemistry; while others, with a greater share of prudence, have either suspended their opinion, or endeavoured to reduce the facts to known theories. Whatever their system may have been, all of them must, however, have set out by a similar research, to ascertain whether it would be practicable to obtain the two gases in separate waters. If these waters are absolutely insulated, the gases do not appear. If they are made to communicate by a metallic wire, there is simply a production of double gas; or, in other words, each extremity of the intermediate wire acts in the portion of water in which it is plunged, as if the wire itself proceeded immediately from the extremity of the pile opposite to the one which communicates with that portion; in such a way as that each portion gives, at one and the same time, the two gases. But if, between the two waters, sulphuric acid be interposed, the gases are produced, each of them on its own side. The result will be the same, if the waters be made to communicate with each other by the means of an organized animal substance, such as the hand. Thus is the production of each of the gases, in separate waters, completely proved.

"It is clear that there are but three possible modes of explaining these facts. Either, in the first place, the galvanic action tends to abstract, in each of the waters, one of its constituent parts, leaving in it an excess of the other constituent part; or, secondly, it decomposes the water, and, permitting the disengagement of one of the gases at the extremity of one of the wires, conducts the other, in an invisible manner, to the extremity of the other wire, to allow it to disengage itself there; or, thirdly and lastly, the water is not decomposed at all, but produces, by its combination with a principle of some kind, proceeding from the positive side of the pile, oxygen gas, and, by its combination with a principle issuing from the negative side of the pile, hydrogen gas.

"The first and second of these opinions were advanced in the class of natural philosophy of the National Institute, the former of them, verbally, by M. MONGE, and the latter, in a detailed memoir, by M. FOURCROY. The third is that of M. RITTER, professor at Jena, and of several other naturalists who have embraced his doctrine. It appears, however, so contradictory to the totality of all the other chemical phenomena, that it would have been almost impossible to have admitted it, provided even no other satisfactory explanation could have been given of the experiment in question.

"The memoir of M. FOURCROY contains the results of a considerable number of experiments which he made conjointly with M. M. VAUQUELIN and THENARD. It combines with a very ingenious explanation of the principal fact, a multitude of circumstances which had not been hitherto known. Our naturalists admit the existence of a particular fluid, which they stile *galvanic*, and the circulation of which is from the positive towards the negative side of the pile. According to them, this fluid decomposes the water in its passage from the positive side. It permits the oxygen to escape in bubbles; but combines with the hydrogen to form a liquid, which crosses the water, or the sulphuric acid, or the human body, to reach the extremity of the negative wire. There it is that the galvanic fluid deserts its hydrogen, and suffers it to escape, in its turn, in the form of gas, while it penetrates, itself, into the wire. The following is the experiment by which our naturalists attempt to prove that such is the secret progress of the phenomenon.

" If, say they, an oxyd of silver, carefully washed, be placed between the two waters, the negative wire, near to which the hydrogen gas ought to have been manifested, does not afford any effervescence; and the oxyd is reduced in part on the side of the positive wire. This, they add, is because the galvanic fluid, charged with hydrogen, loses it in passing through the oxyd, the oxygen of which receives it for the purpose of re-composing the water.

" The reporter, M. Cuvier, now proceeds to observe, that the memoir to which he has thus adverted, is merely the prelude of a more considerable work in which the above naturalists purpose to examine galvanism in each of its respective points of view. They claim the merit of the discovery of one of the most important facts which belong to that science. It was known that, by multiplying the discs of which a pile is composed, the force of the shocks may be augmented, as well as the rapidity of the decomposition of water. Our naturalists were desirous to see what would happen, if the surface of each of the discs were to be augmented. They, in consequence, formed a pile with plates of a square foot. The shocks, and the decomposition of the water, remained the same as with an equal number of small discs; but the combustion of metallic wires was instantaneously effected in a most forcible way; insomuch, that when they were plunged into oxygen gas, they became ignited, and burned with a very bright flame: at the same time that small plates, however numerous, were not productive of any such effect. Thus is the combustion governed by a law relative to the surface of the plates; while the other phenomena depend on their number."

From the united efforts and researches of the above-mentioned naturalists it results:

1st, That the disengagement of the hydrogen at the extremity of one of the wires, may be stopped by throwing into the intermediate water an oxyd of silver.

2dly, That this oxyd is reduced to the metallic state, as is likewise the nitric solution of silver.

3dly, That the effects of the progress of this fluid appear to differ from those of electricity; and that, supposing the latter fluid to act in the same way, it is, at the least, singularly modified by the continuity and rapidity of its progress.

4thly, That large plates, however considerable their dimensions may be, do not augment either the shocks, or the painful sensations which ensue from them; but that they augment, in a very powerful degree, the faculty of ignition, seeing that, at the very instant of contact, they bring about the deflagration of iron.

5thly, and lastly, That the movement of this fluid gives birth to a multitude of phenomena, calculated to illustrate, in a surprizing manner, the principal facts of chemistry, in the study of which the science of galvanism cannot fail to be eminently useful.

The name of M. Biot, a celebrated naturalist, and professor of mathematics at the college of France, has been frequently cited in this work. In a memoir on the movement of the galvanic fluid, lately read before the National Institute, he endeavours to demonstrate, that the diversity of the laws by which the galvanic fluid is governed, in the different apparatuses, results from the form given to them, by virtue of which the celerity of that fluid is either diminished or augmented. The experiments which had been made at the School of Medicine, had proved, in the first place, that there is an attraction between the two extremities of the galvanic pile; and, secondly, that, at each extremity, the particles of the fluid mutually repel each other.

To employ his own words: "These facts

serve as a foundation to the inquiries on which I now enter. It is known that points readily drain off and give out electricity. This property must likewise exist in all fluids the particles of which mutually repel each other. In proportion as the points are blunted, their power is diminished; and, if we follow this analogy, we shall see that plane surfaces, which are, in a manner, the elements of great spheres, must abandon with difficulty, at their superficies, the fluid with which they are charged. In proportion as they become more extensive, this difficulty will be augmented. This property of plates is discoverable in the condenser of VOLTA. The round metallic plate of this instrument retains the electricity, so long as its superficies is applied to the circular piece of marble, and discharges it when it merely touches the latter by its edge. Thus, when a pile is constructed, and its two extremities made to communicate, the motion of the fluid, determined by this mean, must be rapid in proportion as the discs are smaller, and *vice versa*. On another hand, the absolute quantity of the fluid, formed in any given time, diminishes with the surfaces of the discs, all the other circumstances remaining the same. If, therefore, two piles be formed, one of them with large plates, and the other with small ones, they will give, in the same space of time, the former, a greater mass of the fluid, possessing a less celerity of motion, and the latter, a smallèr mass, provided with a greater celerity. Let us examine how the combination of these two elements is capable of influencing ourselves.

"The shocks depend much less on the mass of the fluid, than on its celerity, as is proved by the experiment of the Leyden phial. Thus, what is gained in the mass, by an augmentation of the dimension of the discs, is more than compensated by the corresponding diminution of the celerity. The shocks must consequently be diminished in their intensity, in proportion as the surfaces of the discs are augmented. But, on another hand, they (the shocks) cannot be indefinitely augmented, in proportion as the above surfaces are diminished, because the absolute quantity of the fluid is diminished in consequence, and, with it, the intensity of the shocks. A pile composed of small plates will give a sharper and more intense shock, than another pile composed of larger plates.

"These results, which a simple mode of reasoning points out, are likewise confirmed by experiment and observation. The eight large plates employed by M. Fourcroy, gave a weaker shock than an equal number of ordinary discs, of the size of a crown-piece. An apparatus composed of twelve circular discs of zinc, and of twelve discs of copper, having each a diameter of thirteen inches, scarcely, if at all, excites a tremulous sensation in the moistened hands. It is simply productive of an almost insensible taste in the mouth; and never occasions the galvanic flash. One of these plates is more than equal, in point of surface, to eighty common discs. Lastly, a pile composed of fifty *centimes*[*], and of fifty discs of zinc of the same size, gives a very powerful shock. The flashes of light which are produced by its means, are very brilliant, and are accompanied by a strong and pungent taste in the mouth. These fifty small plates are not, however, more than equal in surface to eight common discs, and, in their aggregate, form only the tenth part of one of the large plates of which mention is made above. When this pile is discharged with large conductors, and that repeatedly, in the space of several seconds, it does not, the instant after, occasion the smallest sensation, either in the person by whom the preceding shocks have been received, or in another individual by whom the communication is suddenly established. The reason of this is, that the absolute quantity of the fluid is very small, at the same time that its celerity is very considerable.

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[*] These small republican coins of copper have a diameter of nearly seven lines English.

"The small apparatus I have just described, has likewise furnished me with several facts, of which I shall speak hereafter. I return to the modifications which are produced in the phenomena of galvanism, by the different proportions of the mass and of the celerity of the fluid. The sensation of taste, and that of the galvanic flash, being obedient to the same law as the shocks, the conclusion which is to be drawn is, that they principally depend on the celerity of the fluid. It is not the same with the sparks, and with the combustion of metals: these phenomena, in the production of which the fluid acts by its mass, and by the continuity of its presence, cannot fail to be singularly favoured by the great plates, if it be granted that they not only augment the mass of the fluid, but also slackens its movement in the pile. This fact experience has confirmed: the great apparatus not only brings about the combustion of iron wires in oxygen gas, but burns them in the open air with an unceasing energy.

"The small apparatus, composed of centimes, affords a small spark, which is lively and brilliant, but which does not produce any effect of a similar kind. The attractions depend likewise on the mass of the fluid, and are accordingly much more sensible with the large plates. In the great apparatus, if the two metallic wires by which the communication is established, be made to touch each other, they adhere together strongly, in spite even of their own elastic force. So great is their adhesion, that they may be moved to and fro', and made to support several small weights, without being detached. This experiment succeeds better when wires are made to touch each other at their sharp points, than in any other manner. It may be tried with metallic substances of every description, provided they be not oxydated. By this adhesion the communication between the two extremities of the pile is established; and so long as it subsists, no other galvanic phenomenon takes place. Two other wires cannot be made to adhere at the same time; and when the connexion between the former wires, provided they are tolerably large, is destroyed, the apparatus remains for some time, before it recovers this property. These wires may remain in a state of adhesion for several hours; probably during the whole of the time of the action of the pile. Two metallic lamina, or thin plates, may likewise be made to adhere to each other, when brought together at their angles. The metals employed by me being classed according to the order of their faculties, to produce these effects, are as follows: iron, tin, copper, and silver. This order is the inverse of that of their conducting qualities.

"The facts which I have stated, appear to me to demonstrate the power of points to emit the galvanic fluid, and that of plates to retain it. The result to which they lead, tends still further to confirm these considerations; seeing that the adhesion of the communicating wires, when they are brought laterally towards each other, must be effected with a facility proportionate to the greater degree of difficulty which the fluid meets with in escaping by their points. I had an opportunity to remark that, in the piles composed of the small plates, the oxydation was carried on with a greater rapidity than when the common sized discs were employed in their construction. This led me to think that the celerity of the fluid might influence the oxydation. To satisfy myself on this head, I made the following experiment.

"Beneath a cylindrical bell-glass, and resting on a supporter fixed to a pneumato-chemical apparatus, I placed a pile composed of thiry-nine discs of zinc, and of as many discs of copper, each of them of the size of a crown piece. I did not establish any communication between the two extremities; and, having observed the time which the water required to ascend into the apparatus, found that it rose eight lines in the space of five hours and a quarter. I now took the same discs, and, after having cleansed them, I placed between them the pieces of cloth which had been before employed, but moistened with a fresh solution. Having constructed the pile in the same way as on the former occasion, I subjected it to the same circumstances, but established a communication between its two extremities. The water now rose to the same height of eight lines in the space of an hour and a half. On persevering in this experiment, the elevation of the water was constantly much more considerable when the communication was made, than in the other case; and the oxydations were obedient to the same law.

" I repeated this experiment with two piles, each of them composed of twenty-two small discs of zinc, and twenty-two *centimes*. Having placed these piles, at one and the same time, beneath two similar bell glasses, and over the same pneumatic apparatus, I obtained results similar to those which attended the preceding experiment. The pile in which the current of the galvanic fluid was established, raised the water, in seven hours and a quarter, to the height to which it was raised by the other pile in eleven hours; and the rest of the experiment made a proportionate progress. At the termination of thirteen hours the water rose, in the former of these piles, to the height of seventeen lines and a half; and in the other, to the height of six lines only. Lastly, when the absorption ceased, it was at the height of twenty-five lines and a half in the former; while in the latter it had risen fourteen lines only.

" On taking in pieces the two piles, and comparing, one after the other, the plates of which they were composed, I observed an infinitely greater oxydation in the one, in the case of which the communication had been established. It follows from hence, that the circulation of the fluid, in the apparatus, augments the oxydation of the metallic discs, as well as the absorption of the oxygen. On another hand, the increase of the oxydation appears to augment the absolute quantity of the fluid which is formed. Consequently, the oxydation of the discs, in the galvanic apparatus, is at once the cause and the effect.

" I shall here notice a fact which had been before observed by my colleague Cuvier and myself, in our experiments on the absorption of oxygen by the galvanic pile. When it is constructed in the following manner—*zinc, water*, and *copper*, and is placed beneath a bell-glass, to prevent the renewal of the atmospherical air, it re-acts on itself, in such a way as that the zinc is constantly perceived to seize on the copper, the copper on the zinc, and thus, consecutively, from the lower to the upper part of the column.

When a contrary disposition is made in the construction of the pile, an inverse action takes place. To be enabled to seize on the copper, the zinc is obliged to permeate the bit of moistened cloth by which the two metallic substances are separated. In the piles in which the communication is not established, this permeation does not take place. The surface of the copper is smooth, and that of the zinc, opposite to it, covered by small black filaments, which follow the direction of the threads of the cloth. When the communication has been established for some time, several particles of the oxyd begin to pass off, and proceed towards the copper. Lastly, when the action is powerful, the surface of the latter metal becomes at length entirely covered over. The action now ceases; and this transmission, by renewing the surface of the zinc, contributes to give a longer duration to the effects of the apparatus. It occasionally happens that the oxyd of zinc, after having permeated the bit of cloth, recovers on the copper its metallic state.

“ When the copper seizes on the zinc, the metallic surfaces are in a direct and immediate contact. In this case, provided the copper adheres to the zinc, it preserves its metallic brilliancy, and is sometimes converted into brass. I have never been able to notice these revivifications, unless when the communication is established between the extremities of the pile. To the end that they may be produced, it is necessary that the round pieces of cloth should neither be too thick, nor of too close a texture. These results demonstrate that when the pile is constructed in the following manner; *zinc, water, copper*; *zinc, water, copper*, &c. the current of the fluid takes a direction from the inferior to the superior part of the pile; but that the contrary happens, when the disposition is made as follows: *copper, water, zinc*; *copper, water, zinc*, &c. This perfectly agrees with the experiments made by VOLTA.

"After having considered the movement of the fluid in the interior of the apparatus, I endeavoured to ascertain how this movement is modified, when water is employed as a conductor. In the experiment I made on this occasion, the apparatus, which remained fixed during twenty-four hours, constantly presented the same phenomena. There were not any bubbles disengaged from either of the plates; neither was there any adhesion of similar bubbles to their surfaces. They did not at the same time afford the smallest trace of oxydation. The pile, which still continued to give shocks, having been taken in pieces, presented appearances similar to those it assumes when the communication is not established between the poles. The plates of zinc were covered by black filaments, which had not been able to permeate the pieces of cloth, to seize on the copper. The quality of the cloth was, however, perfectly well adapted to the transmission. It had been employed on several preceding occasions; and the round portions had been carefully washed and dried, before they had been impregnated with a new solution.

" Water, when placed between the conductors, presents therefore a resistance to the movement of the galvanic fluid. Provided this resistance be too powerful to be overcome by the fluid, the effect is not transmitted; and there is not in reality any communication between the two extremities of the pile.

" In repeating this experiment, I allowed a distance of twenty-one inches only between the plates, and impregnated the round portions of cloth with a strong solution of muriate of soda. The shocks, which were very powerful, and the distinct flashes of light, ceased as soon as the communication was established through the tubes, even when the most sensible organs were made to constitute a part of the chain. But when the water contained in the vessels was raised up by suction into a glass tube, and the extremity of the tongue kept for some time on the small column of fluid withinside, the galvanic taste became very perceptible. This taste became still more pungent with a shorter tube; but did not give rise either to the flash of light, or to the slightest tremulous sensation. These results prove that water is intrinsically but an imperfect conductor of the galvanic fluid.

"What I have just observed must not be considered as establishing a difference between electricity and galvanism: it leads, on the other hand, to the deduction of a new analogy between them. It is true, as I have myself been enabled to ascertain, that under the circumstances which attended the process I employed, it would have been possible to transmit through the water the shock produced by the Leyden phial, notwithstanding it might have lost, in its passage, a part of its intensity. But it ought likewise to be observed, that the inevitable thickness of the glass, requires, for the production of the discharge, the accumulation of a pretty considerable quantity of the electric fluid. Now if, as becomes more and more probable, galvanism is an effect, successively repeated, of a very weak electricity, impelled with a very great celerity, all contradiction will cease; seeing that, in such a case, water must be considered as a very imperfect conductor of electricity.

"It is essential to observe that, in the above cited experiments, the galvanic fluid could not have been transmitted otherwise than through the mass of the water. This would not have been the case, if the communication had been established by the means of an open vessel. The fluid, which would then have been at liberty to glide over the surface of the water, would have been transmitted to a much more considerable distance. A proof of this was afforded to me in a very simple manner, in an experiment in which I had established the communication in a vat more than three feet in breadth, into which I had plunged, at a distance of two feet from each other, the extremities of two metallic conductors, terminated by discs having a diameter of six inches. When one of these thin plates was held in one of the hands, and the water in the vat touched with the other hand, or with the tongue, the shock, the galvanic savour, and the flash of light, were instantly felt and perceived. But when the fluid was raised by suction into a glass tube, the effects ceased as soon as the column of water touched the tongue. It is probable that, had the contact been continued for some time, the galvanic taste might finally have been perceived; but this I did not endeavour to ascertain on the present occasion.

"On repeating the above experiment, I found that the effects of a very powerful pile, which were manifested on the surface of pure water, at the distance of nearly two feet, without having, in this transit, sustained any loss of energy, were reduced to a simple savour, when they were transmitted through a small column of water, of the height of eight or ten lines, raised by a suction as near as possible to the conductor, which was plunged in the vat. When a large humid surface is thus presented to the movement of the galvanic fluid, it flows off in a greater quantity, and its celerity is augmented; since, by this disposition, the oxydations of the discs, of which the pile is composed, become much greater. The property of gliding with facility over the free surface of water, presents a new analogy between the galvanic fluid and electricity.

" In this way may be explained the difficulty which the above fluid encounters in its transmission over metallic conductors; a difficulty which seems to disappear, when its progress is excited, by guiding it, in a manner, with the moistened fingers. It then glides with a great rapidity over the humid surface which the fingers have left on the conductors; while, otherwise, it might have been stopped, or at the least retarded, by a multitude of obstacles, such as the passage from one of the conductors to the other by round surfaces; the oxydation of several points on its route, &c. Obstacles of this nature would not, however, have resisted a more considerable quantity of the fluid, and would have yielded, for instance, to the common quantities of electricity, which we know how to excite.

" This fact, which might have been considered as establishing a very important difference between galvanism and electricity, being referred to the above cause, belongs, on the other hand, to one of the analogies between the two fluids. The property which is possessed by the galvanic fluid, of gliding with rapidity over the free surface of water, likewise explains the augmented action of the moistened conductors, when they are forcibly grasped, to establish their contact with a greater number of points. The fluid, being now at liberty to spread itself over the humid surface presented to it by the conductors, flows in a considerable quantity, and with great celerity. This disposition must therefore augment the effect of its action on us; and the more so in proportion as the surface of the conductors is extensive.

" Lastly, it is owing to the same cause that the galvanic fluid is transmitted more effectually, and to a greater distance, on the parts of the body, when they are moistened, than when dry.

Several other apparent irregularities, which the galvanic apparatus occasionally presents, may likewise be referred to the properties above pointed out."

In this memoir an attempt is made to prove,

1st, That the laws of the movement of the galvanic fluid result from the repulsive property of the particles of which it is composed; and that, in this point of view, these laws are similar to those of electricity.

2dly, That the principal cause of the variations in the phenomena produced by the different apparatuses, consists in the diverse proportions in which the quantity, or mass of the fluid, is combined with its celerity.

3dly, That this fluid, which finds great difficulty in its passage through water, glides over the surface of that fluid with a great facility.

4thly, and lastly, That the chemical effects which galvanism produces, cannot be considered as essentially distinct from electricity; on this account, that the galvanic fluid is never elicited in the apparatuses employed for that purpose, unless with a great celerity, and a small mass; while electricity, when it is put in motion by the means of batteries, has at the same time a great mass, and a great celerity. Now if, in galvanism itself, the different proportions of the celerity to the mass, give rise to such marked differences,

ferences, how much greater must these differences be, between the electricity produced by machines, and the successive impulsions of a very weak electricity acting with a very great celerity?

The above memoir, in bringing forward several facts by which galvanism and electricity are approximated, in as great a degree as their dissimilarity had before appeared to be established by other facts and conclusions, furnishes an additional evidence of the uncertainties and doubts which attend the sciences, so long as their progress is not secured by indubitable experiments and explanations. The stability of these experiments, however, is, as well as the perspicuity and evidence of the explanations, rather the result of time than of reason. The latter oftentimes misleads in the theories it contrives. It is essential, that ideas which had not before presented themselves, and new facts, which are frequently the result of chance, should again lead the naturalist into the track he has abandoned, and shew him how far he has deviated from the truth he sought, while he has flattered himself that it was within his grasp.

An allusion having been made by M. BIOT to the experiments instituted by him, in concert with M. CUVIER, on the absorption of oxygen by the galvanic pile, we insert here an analysis of these experiments, which furnish new and important information on the properties of the apparatus in question: they chiefly refer to the mutual action of the pile and of the circumambient air.

To ascertain the action of the apparatus on the atmospherical air, our naturalists constructed a pile, consisting of discs of zinc, copper, and pieces of cloth, moistened with a strong solution of sulphate of alumine. This pile was placed beneath a bell-glass, of a known capacity, and beneath a pneumato-chemical apparatus. The communication between the two extremities of the pile was established withoutside the pneumatic apparatus, by iron wires introduced into glass tubes, bent, and filled with water. At the expiration of forty-eight hours, the water had risen about five degrees in the bell-glass, and the gas which remained in it, discovered all the characteristics of azotic gas. It was lighter than the atmospherical air; extinguished lighted tapers, &c. &c.

After having ascertained that the oxygen gas had been absorbed by the pile, it was necessary to determine whether it augmented its effects. For this purpose, the same pile was laid over the pneumato-chemical apparatus, in a long and narrow glass. The whole having been covered with a bell-glass, of a sufficient size, and of a known capacity, the communication was established withoutside the apparatus, by the means of iron wires inserted in glass tubes filled with mercury. The water in the large bell-glass was afterwards raised, by suction, to a determinate height. The pile remained in action for the space of seventeen hours. It was ascertained by the absorption, that the air left beneath the bell-glass had now lost its oxygen; and the pile had also lost the whole of its action. Pure oxygen having been introduced within the glass, in such a way as entirely to replace the water it contained, the action of the pile was instantaneously re-established, and became almost as powerful as at the commencement of the experiment. It was still allowed to act for some time; and the absorption again went on as before.

It was demonstrated by this experiment, that oxygen, under particular circumstances at least, tends to augment the effects of the pile. It was still, however, to be ascertained, whether this oxygen is absolutely necessary to the pile, and if it constitutes one of its elementary parts. For this purpose a pile was constructed, and a small apparatus, calculated for the disengagement of bubbles, fitted to it. It was placed beneath the recipient of the pneumatic machine, and the vacuum made with the utmost precision. The bubbles continued to be disengaged, but perhaps not so forcibly as heretofore. This experiment was repeated in a more simple manner, by placing the pile alone beneath a recipient, which had on its summit a metallic rod. This rod, on the one hand, and the body of the machine, on the other, served as conductors; and, nothwithstanding the vacuum was made with every possible care, a powerful shock was felt, at the same time that the decomposition of the water took place. These phenomena being altogether in opposition to what had been advanced by other naturalists, M.M. Biot and Cuvier, without aiming to establish a rigorous comparison between the galvanic and electric fluids, bring forward an experiment well calculated to illustrate the above results. "It is well known," say they, "that a Leyden phial discharges itself beneath the recipient of a pneumatic machine, because, the pressure of the external air being destroyed, the fluid contained in the coating withinside, escapes by the wire, and proceeds to the external surface, which exercises on it an attractive force. When this experiment is made in the dark, streams of light are observed to flow off from the wire, and to take a direction towards the external surface. In our experiment," they add, "the pile is discharged in the same manner. The apparatus by which the disengagement of the bubbles is effected, renders the passage of the fluid manifest, as is the case with the streams of light in the Leyden phial; and this passage is continued, because the pile recharges and discharges itself at every instant," &c. Finally, our naturalists conclude from their experiments, that the galvanic pile has an inherent action, independent of the external air, which may, notwithstanding, augment its force under certain circumstances.

M. GAUTHEROT, a French naturalist who has been very assiduous in his galvanic researches, has recently made several experiments, which are not devoid of interest. Being desirous to try the effect of the Voltaic pile on his own organs, he obtained a very singular result, which had not at that time been noticed. When he applied two moist metallic plates to his cheeks, temples, forehead, and even to his chin, he felt, independently of the shock, the sudden passage of a flash of light, and a very peculiar and painful sensation of burning, which lasted as long as the application of the plates was made to the different parts of the face. When the influence was directed to the cheeks, he felt, besides, in the inner part of the mouth, the savour which is peculiar to galvanism, notwithstanding the contacts were external, and in a manner extraneous to that organ.

Relatively to the electricity which results from the Voltaic apparatus, he hazarded, in the first instance, the following explanation. He observed that, since water, when decomposed, changes its state, and, from being liquid, assumes a gaseous form, this change cannot be brought about without a change in its electrical disposition. From this, and from no other cause, he supposed the electricity which is manifested to result. Having continued his researches on this subject, and perceiving that the metallic plates belonging to the apparatus were strongly oxydated, when it had been for a considerable length of time employed in the experiments, he was desirous to notice, more particularly, the influence of the two metals, silver and zinc, in the decomposition of the water. For this purpose he placed, on the two opposite sides of a disc of zinc, two small bands of pasteboard, to support a disc of silver having a dimension similar to that of the zinc. He now introduced a drop of water between the two discs, in such a way as that it should touch each of the metallic substances. Having examined them from time to time, he could not perceive, at the expiration of three days, the least effect of oxydation; while, in another apparatus, disposed in the same way, with this difference, however, that there was a slight metallic communication between the two discs, the oxydation began to be perceived at the expiration of eight minutes. Here, the oxyd of zinc, although of a specific gravity greater than that of the water, abandons the zinc, which is at the inferior part, to adhere to the silver, on which it describes the circumference of a drop of water. The oxydation is carried on with a promptitude which depends on the nearness of the plates to each other; and *vice versa*.

On the subject of the conducting faculty of water in glass tubes, he noticed a singular fact, namely, that the water contained in tubes which have a narrow opening, is less permeable to the galvanic fluid than when the opening is more considerable.

To ascertain the substances, the conducting faculty of which produces the galvanic savour, the shocks, the sparks, and the decomposition of water, our naturalist contrived a small, but very simple instrument, by which he was enabled to make a trial of various substances, in each of the three states of solid, fluid, and gaseous, in which Nature has supplied them.

By the means of this instrument, he found that a stratum of air of an extreme tenuity, not thicker than a silken thread, as it is spun by the worm, is not in any degree permeable to the effects of a common galvanic apparatus. That flame itself, which had hitherto been considered as a good conductor of electricity, is likewise not permeable to the transmission of the effects of the apparatus. That paper, dried woods, silk, wool, cotton, thread, ivory, wax, oil, sulphur, phosphorus, the oxyds of copper and zinc, the diamond, the sapphire, the ruby, the chrysolite, the agate, and the human skin, are not conductors. That the muriate of soda, and that of ammonia, notwithstanding they augment, in a very considerable degree, the conducting faculty of water, are not conductors in themselves. And that, generally speaking, the substances which have been hitherto found to possess a conducting quality, are very few in number.

It was observed by M. GAUTHEROT, that, in the decomposition of water, charcoal decomposes that fluid in the same way with non-oxydable metals; or, in other words, that when two pieces of charcoal are employed for this purpose, one of them disengages the hydrogen gas, and the other the oxygen. He had occasion to notice this peculiarity, that when the portions of charcoal touch each other in the water, its decomposition is not stopped on that account, as happens when metallic substances are brought in contact under the same circumstances. Indeed, if, to bring them more immediately together, one of the pieces of charcoal be cut in a furcated shape, this does not become an obstacle to the decomposition of the water. This proves, according to his mode of reasoning, that charcoal is not so powerful a conductor as the fluid on which it acts.

The next inquiry made by our naturalist, was whether a pile could be constructed by the means of charcoal, and pyrites, or sulphuret of iron, or carburet of iron, or, lastly, plumbago. After many ineffectual attempts, he contrived an apparatus of forty layers of charcoal and plumbago, which afforded a strong and pungent taste, accompanied by the galvanic flash of light, and, finally, produced the decomposition of water, the charcoal side disengaging the hydrogen gas. This latter circumstance, he observes, removed every doubt of the influence of metals, even of that of the iron which might have been contained in the plumbago; since, supposing it to have existed there, the hydrogen gas would have been disengaged on the side of the plumbago, in the same manner as it is disengaged in batteries constructed with charcoal and iron.

In continuing his researches, not with the pile, but with the apparatus composed of a range of cups, he found that, by the introduction of the extremities of two wires of silver, or platina, into his mouth, at the same time that the other extremities were plunged in the first and last of the cups, the galvanic savour was still perceptible after they had been withdrawn from thence, and had been brought in contact. It was in some degree permanent while the contact was continued; and was renewed at intervals, when they were brought together, after having been withdrawn from each other.

He observed that the galvanic savour was still more perceptible, when the extremities of the two wires were introduced into a bottle of water impregnated with salt, and prevented from touching the fluid by the means of a cork. In this case, by plunging the two other extremities into the apparatus of the range of cups, or by bringing them in contact with the two extremities of a common pile, more especially at the moment when the water in the bottle was decomposed, the savour was strongly manifested as soon as the two extremities which had been made to communicate with the apparatus, were introduced into the mouth. It was even accompanied at times by a slight shock; and its activity was of a much longer duration. By the means of this new apparatus water was decomposed.

This experiment, which is not to be explained by the theory of electricity, appeared to our naturalist to be extremely interesting. As it is susceptible of several modifications, he is of opinion that it may probably be the source, or basis, of several others, which may lead to the discovery of the theory of this new branch of natural philosophy.

In another experiment he plunged the two extremities of a single platina wire into the first and last of the cups in the above-mentioned apparatus; and, having brought the two ends of the wire near to each other, without, however, allowing them to touch, perceived, as soon as he had introduced them into his mouth, the galvanic savour, the strength of which he found, by several other similar experiments, to be proportionate to the diameter of the wire.

It is not essential to the success of this experiment, that the two cups at the extremities of the range should contain water impregnated with salt; and, indeed, in this case the saline solution might occasion some doubt relative to the cause of the savour. To remove every uncertainty on this head, and to give a greater degree of simplicity to the experiment, M. Gautherot filled with distilled water two cups, between which, and the extreme cups of the apparatus, he established a communication by the means of two platina wires. He then plunged into the cups which contained the distilled water, the two extremities of the platina wire by which the savour was intended to be produced; and, having brought them into contact with those which answered the purpose of conductors, waited the disengagement of the bubbles arising from the decomposition of the water. By this mode he obtained the maximum of the savour which is furnished by experiments of this description. He could not bring himself to agree with VOLTA, that it is to be ascribed to an effect either of acid, or of alkali, resulting from the decomposition of the water; since, when the two extremities of the wire, after having been taken out of the cups, were plunged into pure water, it was still very perceptible. By way of a comparative experiment, he plunged the two extremities of a platina wire, one of them into the nitric acid, and the other into an alkaline solution, and, having afterwards immersed them in a glass of water, this immersion sufficed, not only to disengage them from these very active agents, but likewise to prevent the production of the slightest savour in the mouth. This experiment merits, in his opinion, a very serious investigation.

Several naturalists, he observes, are of opinion that an unknown agent is combined with the electric fluid, in the production of the galvanic phenomena. Their persuasion is founded on this particular circumstance, that several of the galvanic phenomena do not accord with the explanation which has been given of them by the known laws of electricity. The experiments which have been detailed above, seem to favour this opinion. It is certain, at the least, that the decomposition of water, by the apparatus contrived by M. GAUTHEROT, does not correspond with what has been hitherto ascertained relatively to the properties of the electric fluid.

We have thus given the substance of several memoirs, by the above naturalist, which were read before the National Institute. A favourable report was made on them by M. M. FOURCROY and VAUQUELIN; and from this report we extract the following passages.

" In the first of the memoirs which we have examined, and which contain several facts of great importance in the history of the galvanic fluid, the author gives a description of a new instrument he has contrived, to measure the conducting faculties of different natural, solid, liquid, and even gaseous bodies. In this memoir the whole of the theory and practice of the different kinds of conductors is detailed. From the result of his experiments he draws several consequences which seem to disprove the identity of the electric and galvanic fluids.

" The object of the second memoir comprehends the galvanic properties of charcoal, considered more particularly as a conductor. Our author shews that it is not so perfect a conductor as metallic substances.

" The third memoir is very curious, and *unique* in its kind, insomuch as it ascribes to charcoal a property which it was not known to possess, that of forming with zinc a galvanic apparatus, composed of seventeen layers, which produces slight shocks, the decomposition of water, &c.

" In his fourth memoir, M. GAUTHEROT follows up his researches on the substances which are conductors of the galvanic fluid, and points out those which ought essentially to be considered as such. He describes the composition of the different galvanic columns, and the mode of their construction.

" In his fifth, and last, memoir, he details his attempts, which were, in the first instance, ineffectual, to construct a galvanic apparatus without metals; and announces that, after many researches, he succeeded in accomplishing the aim he had in view. Charcoal and plumbago are the elements of this pile. This new fact, which exclusively belongs to M. GAUTHEROT, cannot fail to contribute essentially to the ascertainment of the properties and nature of the galvanic fluid."

We extract the following reflections and observations on galvanism from a very ingenious work on the principles of physiology, by M. DUMAS, a celebrated French professor.

After having demonstrated the existence of the electric fluid, contained in all the parts of the human body, more particularly in the nerves and brain; and after having proved that, by its constant tendency to place itself *in equilibrio* with the electricity possessed by the bodies by which it is surrounded, it is itself the physical cause of all the electric phenomena, to which man, whether in a healthy state, or in a state of disease, can be subjected: after having observed, without adopting, however, the miraculous relations of cures on which credulity has bestowed too implicit a faith, that it is impossible not to comprehend this powerful means of nervous excitement among those which hold out great and peculiar advantages in the treatment of paralytic and convulsive affections; our author adds, that the new class of phenomena, disclosed to us by the experiments of GALVANI, VALLI, FOWLER, WELLS, BERLINGHIERI, HUMBOLDT, &c. are to be derived from the same principle of excitement in the system.

"Has it not been seen," observes M. DUMAS, "that the movements produced in the muscles, by the action of the nerves subjected to the influence of metallic substances, follow pretty nearly the laws by which the usual effects of electricity are governed? The disagreeable sensation impressed on the tongue by the contact of two different metals, had been long known. HUNTER had announced that, by laying a metallic substance on the upper lip, and another on the tongue, the sensation of a flash of light, extending over every part of the face, was to be produced. These facts, which were lost sight of by naturalists, ought to have led them into the track of a discovery, the physiological utility and importance of which will never, perhaps, be equal to all the noise it has made in the philosophical world. I speak of the experimental process which consists in the excitement of the action of the sensitive and motory forces, by the medium of metallic substances. In the explanation of this process, an invisible agent, a new cause, was in the first instance introduced, notwithstanding it appears to be nothing more than an extra mean of experimenting, superadded to so many others, on a class of phenomena anciently known."

The general consequence which the naturalists of Italy, Great Britain, Germany, and France, drew, after having repeated the experiments of GALVANI, was, that the metallic influence is not imparted to the muscles, until after it has been communicated to the nerves. This conclusion appears to have been dictated by the effects to which the nervous parts, as well as the organs of the sensations, are subjected in galvanic experiments. It receives an additional support from the assurance given by HUMBOLDT, that it is impossible to produce the contractions, in a portion of muscle prepared in such a way as to divest it entirely of every nervous ramification. It is, notwithstanding, certain, that the coatings of the muscles alone have been sometimes efficacious in the production of the galvanic effect; and this would be an incontestible proof of an irritation independent of the nerves, if it were not likewise certain, that this irritation is capable of penetrating into the internal tissue of the organs, in which the nerves are concealed. It would, however, be too rash to circumscribe to the nervous and sensible fibres, the impression of the metallic stimulus, seeing that, independently of several rational arguments which prove the contrary of this assertion, the experiments made by Dr. Fowler demonstrate that the action of galvanism is directed at least as much to the vascular, as to the nervous system.

On the subject of the opinion of the latter physiologist, as well as of Humboldt, that galvanism and electricity have not an exact similitude, and that, consequently, the effects of the former of these principles are ascribable to a new property inherent in metals, before unknown, M. Dumas asks the following question: "Where would it be possible to stop, if as many conjectures, and as many principles, were to be allowed, as there are facts to explain? If it be true," he adds, "as these naturalists have asserted, that electricity and galvanism are two separate and distinct causes of phenomena, we have reason to expect, from the progress of human intelligence, that they will be hereafter united and confounded in a third cause, which will embrace both of them, and will produce its effects by different modifications."

M. Richerand, the author of an elementary work on physiology, has published, in the memoirs of the Medical Society of Emulation in Paris, several galvanic experiments and observations, which we shall now proceed to examine. Shortly after the discovery made by Galvani had been communicated to the scientific world, he instituted a series of experiments tending to confirm the theory of our learned countryman, Dr. Millman, on the sea scurvy, and on putrid fevers. The principal characteristic of these affections, is an extreme diminution of the contractible faculty of the muscles, destined for the vital functions, and for the voluntary movements. This had led to a presumption that, in the corpses of those who had fallen victims to the above diseases, the muscles would be but little susceptible to the galvanic stimulus. It was considered, on the other hand, that the galvanic susceptibility would be more strongly marked, and more durable, in the instances in which death had been brought on by inflammatory affections. The object of the experiments, the results of which are given by M. RICHERAND, was to ascertain the degree of confidence to which these experiments are entitled.

"Whatever," he observes, "may be the disease of which the subject of the galvanic experiment has perished, the contractible faculty is alike extinguished in all the muscular organs, as soon as the time, necessary to the ascertainment of the total destruction of the vital functions, has elapsed. I took, indiscriminately, the dead bodies of those who had died of scorbutic or inflammatory affections, without a regard either to the age or sex *. To the nervous and muscular parts of these corpses I applied the metals which produce the most powerful excitement, such as zinc and tin. I brought these metals in contact over large surfaces, but could not obtain the smallest contraction. I afterwards repeated these experiments on a great number of dead bodies, the muscles of which I sprinkled with the oxygenated muriatic acid, a preparation so well calculated, according to HUMBOLDT, to restore their languid and feeble circulation. The result was, however, the same as on the former trials.

"My next experiment was on a young man

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[*] These experiments were made in the hospital of *la Salpetriere*, in Paris, in the presence of M. PINEL, a professor belonging to the School of Medicine.

who fell from the roof of a very lofty house, and fractured the skull, the thighs, and the arms. The liver was lacerated; the abdominal *vena cava* ruptured; and there was a considerable effusion of blood in the abdomen. Notwithstanding the wounded man died on the spot, he was carried to the hospital of *la Charité*. The vital heat was extinguished; but the muscles were totally insensible to the galvanic stimulus.

"Several warm-blooded animals, which had been recently killed, but the heart of which still contracted, at the same time that the vital heat was not diminished in any particular degree, did not present any galvanic phenomena, although nothing was omitted which could contribute to their production. The life of all these animals terminated by convulsive movements, which were more or less powerful, according to their age, their strength, and their size."

It appears probable, from the above observations, that whatever portions of muscular irritability may remain on the approach of death, they are consumed in the last efforts of the vital force. It has been observed by Grimaud, that mortal diseases, for the greater part, terminate by convulsive movements, unless the patients have been debilitated by a low diet of long continuance, or by extreme old age; as if, according to the opinion of Stahl, each animal had received from Nature the sum, or quantity of the movements necessary to the development of the duration of its life; and as if these movements press rapidly and tumultuously, when the termination of its existence is hastened by an accidental cause.

If a limb be severed from a living animal, the muscles are sensible to the action of the galvanic stimulus. But they are insensible to it when the amputation is made after the convulsive movements by which the last agonies are terminated. A leg having been amputated, in the case of a disease of the articulation of the knee, its muscles, and the sciatic-popliteal nerves were laid bare immediately after the operation. The galvanic phenomena were, in this instance, very sensibly displayed; and did not cease until the total extinction of the vital heat.

" These facts, observes M. RICHERAND, seem to prove that the muscular contractibility is destroyed, by the convulsive movements in the midst of which warm-blooded animals expire. In cold-blooded animals the contractibility is more vigorous, and more durable. If this quality can be acted on by the galvanic *stimuli* a considerable time after death has taken place, and at the moment even when the putrefactive process has commenced, is it not because, in these animals, life is less a whole, if the expression can be allowed, but because it is more divided in different organs, which have less need of acting on each other, in the production of this surprizing phenomenon?

"Irritability is of two short a duration in man, to allow the galvanic experiments tried after his death, to furnish any information relative to the greater or less degree in which this vital property is weakened and exhausted, in different diseases. The writers, therefore, who have advanced that the galvanic susceptibility is more effectually extinguished in subjects whose death has been occasioned by scorbutic affections, than in those who have sunk under the attacks of inflammatory diseases, have hazarded a very improbable conjecture, which has not been confirmed by experience."

In a work recently published, entitled, "New Elements of Physiology," M. RICHERAND has introduced an article on galvanism, in which he traces the progress of the discovery up to the present time. Of this article we now proceed to take a short review.

"The name of animal electricity," he observes, "was soon changed for that of metallic irritation *(irritamentum metallorum)*, a denomination which is evidently defective, since it tends to lead to a persuasion, that the irritation excited by metals, is alone capable of producing the galvanic phenomena, at the same time that charcoal, water, and many other substances, are equally favourable to their production. Notwithstanding there is a great analogy between the effects of electricity and those of galvanism, the name of animal electricity has been also renounced, to perpetuate the memory of the celebrated naturalist by whom the discovery of the latter principle was made."

Our author now proceeds to describe what is essential to the production of the effects, in the formation of a galvanic circle. He describes the experiments which an individual may try on himself, such as those made by HUMBOLDT, together with the mode of constructing the exciting arc. He points out the relations which subsist between galvanic susceptibility and muscular irritability. He shews that the former is extinguished in warm-blooded animals, in proportion as the vital heat is dissipated; and that it is more durable in animals with cold blood. In proof of this, he adduces the following facts, which were communicated to him by Professor PFAFF, and are but little known.

"The galvanic chain does not produce any sensible actions, that is, contractions, unless at the moment when it is closed, by establishing a communication between the parts of which it is constituted. During the whole of the time that it remains closed, that is, during the interval that the communication continues to be established, a perfect tranquillity prevails, notwithstanding the galvanic influence is not suspended. Indeed, the excitability is in a very extraordinary degree either augmented or diminished, in muscles which have remained for a long time within the galvanic chain, according to the difference of the situation of the combined metals. If the silver be applied to the nerves, and the zinc to the muscles, the irritability of the latter is augmented in proportion to the time they have remained within the chain. By this mean, it is possible to stimulate to action, and, in a manner, to revivificate, the thighs of frogs, which are again obedient to the *stimuli*, after they have ceased to be moved by them. By distributing the metals in an inverse manner, that is, by applying the zinc to the nerve, and the silver to the muscles, an effect of a diametrically opposite nature is observed. The muscles, which, on their being introduced into the chain, had manifested the highest degree of irritability, appear to be completely palsied, after they have remained for some time in that situation.

" It is very evident that this difference depends on the direction of the galvanic fluid, determined either towards the nerves or the muscles, according to the manner in which the metals are disposed. This fact is of the utmost importance, in the application of galvanism to the cure of diseases. In the cases which indicate the necessity of augmenting the enfeebled irritability, it is preferable to employ the tranquil and lasting influence of the galvanic chain, by distributing the silver and the zinc in such a way, as that the former of these metals may be near to the origin of the nerves, while the latter is laid on the muscles, the benumbed, or totally suspended action of which is to be revived, than to resort to its sudden, transitory, and instantaneously stimulating influence."

The above article on galvanism is terminated by the description of the Voltaic apparatus, and by the explanation given by FOURCROY of the manner in which this apparatus acts, in bringing about the oxydation, and the disengagement of the hydrogen gas. He ascribes this phenomenon to the decomposition of the water by the galvanic fluid. This fluid, according to him, abandons the oxygen to the wire by which the positive extremity of the apparatus is touched, and afterwards conducts the other gas, in an invisible manner, to the extremity of the other wire, to allow it to disengage itself there. This opinion, which is supported by a great variety of experiments, appears to our author to be the most probable of any that have yet appeared.

Several galvanic experiments and observations have been recently published by M. Bichat, physician to the Hotel Dieu in Paris, and author of a very celebrated work on anatomy. In speaking of the influence of the destruction of the brain on that of the heart, after having proved, conformably to observation and experience, that it is not immediately by the interruption of the cerebral action that the heart ceases to act, he confirms this fundamental *datum* of physiology and pathology, by a series of galvanic experiments, which demonstrate that the heart is in all cases independent of the brain.

"These experiments," he observes, "were made by me with the most scrupulous attention, because several very respectable authors have recently advanced an opinion contrary to mine, and have endeavoured to prove that the heart, together with the other muscles of organic life, do not differ, as to their susceptibility to the galvanic influence, from the different muscles of animal life. I shall begin by a detail of the observations I have made on animals with red and cold blood.

"1st, In several experiments made on frogs, I coated the brain, on the one hand, with lead, and the heart and muscles of the inferior extremities, on the other, with a long lamina of zinc, the upper end of which touched the heart, and the lower end the muscles. Having, by the means of silver, established a communication between the coatings of the muscles and those of the brain, the movements of the limbs constantly followed; but I could not perceive any acceleration in the contractions of the heart, when it still continued to beat; and when its action had entirely ceased, it did not display the smallest movement. Whichever may be the voluntary muscle that is coated at the same time with the heart, with a view to a comparison of the phenomena they exhibit at the moment of the metallic communication, there is constantly a marked and decided difference.

“ 2dly, In the case of other frogs, I coated, with a common metallic wire, on the one hand, the cervical part of the spinal marrow, in the upper region of the heart, to the end that the coating might be above the part where the nerves which proceed from the great intercostal nerve, and thence to the heart, originate; and, on the other hand, the heart, and any one of the voluntary muscles. I constantly noticed a result similar to the one which attended the preceding experiment, whenever the communication was established. There were invariably violent agitations in the voluntary muscles, without any visible alteration in the contractile movements of the heart.

" 3dly, I endeavoured to denudate the nerves which lead to the heart of frogs. Several greyish filaments, scarcely perceptible, with the nature of which, I must acknowledge, I am not positively acquainted, were coated with a metallic substance, at the same time that the heart was made to rest on a substance of a similar nature. When the communication was established by the means of a third metal, not the smallest sensible effect was to be perceived.

" It appears to me that these trials, which had been partly made before I engaged in them, are well calculated to decide, in a positive manner, whether the brain has any direct influence on the heart, more particularly when they are repeated with the precautions which were taken by me, to coat successively, and alternately, the internal and external surfaces, and the substance even of the latter organ. In each of these experiments the natural disposition was preserved between the different parts which serve to unite the heart with the brain.

" 4thly, There is another mode of experimenting, which consists, in the first place, in detaching the heart from the breast. Secondly, in bringing it in contact with two different metals, at two points of its surface, or with portions of flesh coated with metals. And, thirdly, in establishing a communication between the coatings, by the means of a third metallic substance. In these cases, HUMBOLDT observes that he has seen the movements produced. I must confess, notwithstanding, that although I have frequently repeated these experiments, with a strict adherence to the forms prescribed, I could never perceive any such result. On other occasions, it is true, a slight movement, very different from the natural contractions of the heart, was manifested, and seemed to be ascribable to the galvanic influence. I should have been inclined to consider this movement as the effect of the mechanical irritation of the coatings, had it not been for the respectable authority of HUMBOLDT, and of several other profound naturalists, who have recognized, in their experiments, the influence of galvanism on the heart, when the application is made in this manner. I am, however, far from pretending to have seen more distinctly, in the trials I have made, than those who have pursued the same track of investigation. I merely state what I have observed.

" 5thly, It may not be improper to observe in this place, that the experiments in which the coatings are not applied, on the one hand, to a portion of the nervous system, and, on the other, to the fleshy fibres of the heart, do not appear to me to be very conclusive, in deciding whether the influence which the brain exercises on that organ, is direct. What rigorous induction can, indeed, be drawn from movements produced by the coating of two fleshy portions?

" I proceed now to experiments made on animals with red and warm blood. It became the more necessary to try them, because the mode of the contractibility of animals with red and cold blood, differs essentially, as is well known, from that of animals with red and warm blood.

" 1st, In the course of the winter of the year 1798, I was authorized to make different experiments on the bodies of the unfortunate persons who had been guillotined. These bodies were brought to me about twenty minutes after the execution had taken place. In several of them the motive faculty was extinguished; while, in others, I was enabled to excite it, with a greater or less degree of facility, in all the muscles, by the ordinary methods which are resorted to on these occasions. It was more particularly manifested in the muscles of animal life, by the means of the galvanic processes. I could never succeed, however, in the production of the smallest movement, when I coated, either the spinal marrow and the heart, or the latter organ and the nerves it receives from the ganglions, by the great intercostal, or from the brain, by the *par vagum*. Notwithstanding this, when mechanical exciters were applied, in a direct and immediate way, to the fleshy fibres, contractions ensued. Did this arise from the nervous filaments of the heart having been for some time insulated from the brain? But, in such a case, why did those belonging to the voluntary muscles, which were likewise insulated, favour the production of the galvanic phenomena? The following experiments will clear up this doubt.

"2dly, In experiments made on dogs and guinea pigs, I coated, with two metallic substances, in the first place, the brain and heart; next, the trunk of the spinal marrow and the latter organ; and, finally, that organ and the nerve of the *par vagum*, from which it receives several nerves. The two coatings having been made to communicate, there was not the smallest apparent result: I could not perceive that the movements were re-produced, after they had ceased; nor that they were accelerated during their continuance.

"3dly, The cardiac nerves of two dogs having been coated, both in their anterior and posterior filaments, another coating was applied to the heart, sometimes at its anterior surface, and at others at its posterior surface, and at others, again, at its *fasciculi*. The communication, as in the preceding experiment, did not produce any apparent movements. In experiments of this description, the communication ought not to be made until a certain portion of time has elapsed after the coating has been placed on the heart, to the end that what is merely the effect of metallic irritation, may not be ascribed to galvanism.

" 4thly, HUMBOLDT observes, that when the heart is suddenly detached from the cavity of the pericardium, taking care that several of its insulated nerves should still remain attached to the viscus, contractions may be produced by coating the latter with a metallic substance, and by touching the coating with another metal. I tried this experiment repeatedly, but without success, except in one particular instance, which was attended by feeble contractions.

" 5thly, On the other hand, I succeeded, almost invariably, in producing contractions in animals with red and warm blood, when I took out the heart, and brought it in contact, at two different points, with metallic substances, between which a communication was established. It appears to me that this is the only mode of producing on that organ, efficaciously and demonstratively, the galvanic phenomena. It does not, however, prove what is the object of my present researches, namely, whether the brain has a direct influence on the heart.

" I repeated each of the above experiments on galvanism a considerable number of times, and with the most minute precautions. Notwithstanding I did not obtain the same results, I would not have it understood, as I observed before, that I wish to throw any doubt on the fidelity of the experiments which have been tried by others. The effects of those which have for their object the vital forces, are well known to be extremely variable. Admitting, even, results different from those which I obtained, it will still be acknowledged, at least such is my persuasion, that, on the score of galvanic excitement, there is an enormous difference between the muscles of animal life and those of organic life. There is not any method so well calculated to ascertain this difference, in experiments on the heart and on the intestines, as that of constantly coating with the metal which is employed as a coating for these muscles, one of those belonging to animal life, and thus establishing a parallel between them.

"If, indeed, we grant that the galvanic phenomena have, on these two kinds of muscles, an equal influence, what is proved by this fact? Nothing more than that these phenomena are, in their succession, governed by laws which are altogether different from those of the phenomena of the nerves with which these muscles correspond."

Having discussed this important physiological question, M. BICHAT proceeds to treat of the influence the destruction of the vital energy of the brain has on that of the respective organs, which he divides into those of animal life, and those of organic life. He inquires, in the first place, whether the interruption of the organic functions, is a direct effect of the cessation of the cerebral action. After having proved, as he had already done in the preceding article, both by observation and experience, that all the internal functions are, in the same way with the action of the heart, withdrawn from the direct ascendancy of the brain, and that, consequently, their interruption cannot be immediately derived from the destruction of that organ; and after having established, anatomically and substantially, that the organic functions are not subjected to the immediate influence of the brain, seeing that the greater part of the viscera by which these functions are performed, receive few, if any, of the vertebral nerves, but are rather supplied with the nervous filaments arising from the ganglions, such as are observed in the liver, kidnies, pancreas, spleen, intestines, &c. he demonstrates the truth of the principle he has established, by experiments on living animals.

These experiments were made on the stomach, intestines, bladder, uterus, &c. of animals with warm blood, and were constantly attended by a result similar to the above. They were afterwards repeated on cold-blooded animals ; and justified the general inference which he draws, namely, that, " with relation to the galvanic fluid, as well as in every other point of view, there is an enormous difference between the muscles of animal life, and those of organic life."

PART IV.

CHAP. XVII.

General introductory observations.

HAVING thus brought down to the present time, an historical account of all the theories and experiments relative to the newly discovered principle of galvanism; and having traced the science from the era of its discovery, through all its successive stages, it now remains for me to arrange the scattered sentiments into a regular order, so as to bring into some kind of system the subject treated of.

Hitherto I have confined myself to a detail of the experiments of the different philosophers, chiefly arranged as to the time when they were performed. It will now be requisite to class under their respective heads, the various discoveries which have been made, commencing with the simple state of galvanic combinations, and rising gradually to the more complicated arrangements.

It is necessary to observe, that the subsequent elements are not a mere repetition of what has already been given. I presume that, on the other hand, they will be found different from any which have yet been offered to the public. Although, in my general sentiments as to galvanism, I coincide in opinion with VOLTA, still in many instances it will be found that I have materially differed from him. I have attempted to prove, that the principles of galvanism and electricity are identically the same. That the former is the evolution of electricity from conducting bodies, forming one of their constituent parts, and disengaged by a chemical process; while the latter is the same principle, rendered apparent to our senses, by the temporary changes of non-conducting bodies, to a conducting state.

As I have undertaken to explain all the phenomena of galvanism on electrical principles; and as, relatively to these principles, I have entertained opinions different from those generally received, I have deemed it necessary to previously state them, seeing that, otherwise, the explanation of the galvanic operations would have been obscure.

It will be seen, that all the operations of electricity are reducible to the action and re-action between air and the electric fluid; and it appears to me, that the phenomena which have been generally ascribed to the influence of such imaginary powers as those of *attraction* and *repulsion*, are here satisfactorily accounted for on simple mechanical principles. When we reflect that electricity is a principle which has been known for more than two thousand years, and that it is still very imperfectly comprehended, it cannot be expected that, at so early a period after the discovery of galvanism, any thing like a perfect system can be formed. I believe the present to be the first attempt to arrange systematically this new branch of natural philosophy, on which account I flatter myself I shall meet with every indulgence, if the work should be found not to be so perfect as it has been my wish to render it. Those who have been habituated to philosophical pursuits, will be fully aware of the arduousness of the task which aims at the reduction of disjointed members into one perfect body. In such an attempt, it is not only requisite to repeat with care the numerous experiments which have been already performed; but also to institute new ones, in order if possible to ascertain certain precise limits. Thus, in the chapter on galvanic surfaces, it appears that it had been admitted as an axiom, drawn from the experiments of FOURCROY, VAUQUELIN, and THENARD, that the shock is in proportion to the number of the plates, and not to the surfaces. In the examination of this fact, I have shewn that there are limits, which, *a priori*, we could not have conceived. These very limits explain to us an important part of the science of galvanism.

What has hitherto been given, is merely to be considered as an historical detail; but this fourth, and last part, constitutes the ELEMENTS of ELECTRICITY and GALVANISM.

CHAP. XVIII.

The principle of galvanism considered as an important agent of Nature—Observations on the systems of MESMER *and* PERKINS—*What is meant by conducting and non-conducting substances, in electrical and galvanic experiments.*

ALL the knowledge of natural philosophy which can be acquired by cursory reading, without the assistance of mathematical learning, must consist in an acquaintance with leading facts and general conclusions: facts are, as it were, the materials of science, and much praise is due to those who have increased the public store, by new experiments accurately made and faithfully related. But it is not in the mere knowledge, nor even in the discovery of facts, that philosophy consists: one who proceeds thus far is an experimentalist, but he alone, who, by examining the nature, and observing the relation of facts, arrives at general truths, is a philosopher.

It is now about thirteen years since an apparently new branch of natural philosophy has been added to the stock of scientific acquirements, and, from the peculiarity of its phenomena, has excited the general attention of philosophers. The circumstances which led to this important discovery by GALVANI, have been already particularized. The experiments of the Continental and British philosophers, have at once developed the influence of this surprizing principle, and have demonstrated its effects as an important agent in Nature.

The peculiar powers of this principle, in rousing into action the dormant energies of vitality, and giving life to parts which were in a torpid inactive state, at first induced an opinion, that it is a principle *sui generis*, and more subtile than that of electricity, which it more particularly resembles. When we reflect, that electricity is a principle known for more than 2000 years, and yet that our knowledge of it is very imperfect, how little ought we to expect from the science of galvanism, in so early a stage as the present period. Galvanism is not one of those mysterious principles which influence the animal frame, without affecting the external senses—its wonderful agency is evinced by its powers of decomposing inanimate matter; and the celebrated GUYTON thinks, that at some period it will be found one of the most general mineralizing principles. When an animated body is subjected to its action, all those effects arise which might be expected from the stimulating power of an active principle. It does not act by influencing the imagination, which has been so many times acted upon by the magnet and the tractors.

It is true, that in a machine so complicated as the human body, subject to so many strong impressions, and to the almost absolute power of imagination, the changes said to have been brought about by the votaries of MESMER or PERKINS, might have been produced by causes very different from the agency of the loadstone or of the tractors: these circumstances have been more vaunted than examined by the advocates of animal magnetism and perkinism. MESMER, we know, pretended that when he was himself touched by the loadstone, he had the capacity of transmitting by his finger the magnetic streams from his own body to that of his patient. STEIGLEHNER proved all MESMER's trials to be illusive and fallacious; and a highly respectable character, Dr. HAYGARTH, has placed in their proper light the delusive tractors. May we not, however, make some allowance for those intelligent persons who have been misled by the visionary effects imputed to animal magnetism and to the tractors, when we consider that two such celebrated characters as BACON and BOYLE were contaminated by the prevailing enthusiasm of the ancient magnetists? MESMER pretended, that the nature of the inherent power on which he expatiated, was only known to himself, and by him alone it could be communicated. It is also said, that the tractors lose their virtue if they fall into other hands than those of the purchasers *.

In the author's inquiries into the surprizing principle of galvanism, no deductions will be made

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* In the Mesmerian doctrine there is something fascinating, something calculated to strike the minds of the multitude: an universal fluid, subtile beyond comparison, capable of receiving, propagating and communicating all the impressions of motion, and constituting a mutual influence between celestial, terrestrial, and animal bodies.

It is difficult to conceive what interest can be attached to the tractors, which are founded on no medical or philosophical principles, and the prescribed mode for the application of which is so truly ridiculous, as to render it suspected, that the imagination must be very easily acted upon, if capable of being thus influenced. It has been, however, asserted, that they cure by their own inherent power; and proofs have been brought of their surprizing efficacy in the recovery of dumb animals. It must be in the recollection of many medical men, that during the Mesmerian rage, the Count DE PUYSEGUR's dog was said to have been cured by magnetism—the case is as follows: The dog happening to stand in the way of a young man, the latter took hold of him by the tail, and whirled him to a little distance; the suddenness and violence of the motion deprived the animal of his senses, and this insensibility continued long enough to admit of the ceremonies of magnetism being performed, and to enable the Count to boast of the cure.

but what experimental facts authorize; no assertions advanced but what can be demonstrated. Although he may occasionally indulge in the wide field of conjecture, the opinions which will be then brought forward will only be noticed as hypothetical, and as affording a presumption that they may occasionally lead to the ascertainment of some important facts.

As all the galvanic phenomena appear to him to accord with the principles of electricity, and to be regulated by the same laws—as his opinions upon these laws differ from those generally received, he has thought it advisable to premise them by a few introductory remarks.

The observations he has made as to the galvanic influence in medical cases, are comparatively but few; its influence in cases of deafness he has frequently experienced. In recommending the employment of this principle to the serious attention of practitioners, he hopes it will not be understood by the public, that he confines his attention solely to this practice; it is only one of the branches of that course of science of which every practitioner ought to be master*.

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* There is a remarkable instance of an eminent practitioner, who, from his extensive acquired knowledge, deservedly ranks high in the profession, but who found it prudent

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In the employment of this principle, the same precautions are requisite as in the exhibition of any active medicine: the sensibility of the part affected, and whether it should be directly or indirectly acted upon, are circumstances to be only regulated by the judicious practitioner.

CONDUCTORS AND NON-CONDUCTORS OF ELECTRICITY.

The experiments of Mr. Gray casually led to this grand division of all substances, as they relate to electricity. They were by him termed electrics and non-electrics, from a supposition that the

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to decline being thought to have paid any peculiar attention to a distressing malady, which, in the course of his enlarged inquiries, he had greatly benefited.—Medical persons are too generally deemed not to require such a philosophical education as may render their knowledge of Nature more perfect; even by some, common sense is deemed incompatible with medical studies.—A French lady being told that her physician had not common sense, replied, "Tant mieux; un homme qui passe son tems à étudier le sens commun, comment peut-il apprendre la medicine? Monsieur l'Abbé qui parle Grèc comme Homère, ne sait pas danser:" and Dr. Moore tells us a ludicrous anecdote of a French lady who had injured her ancle—her practitioner ordered a poultice to be boiled precisely four minutes and a half: upon Dr. M. expressing some surprize at the minuteness of these orders, she exclaimed "Mon Dieu, quelle precision! il calcule comme un ange!"

former alone contained the principle of electricity, as being only apparent in consequence of their excitation. Dr. FRANKLIN, from some experiments, being induced to suppose that electricity is equally diffused, changed the terms of *electrics* into *non-conductors*, and *non-electrics* into *conductors*. From some experiments, I am induced to suppose, that electricity is universally diffused, but not equally; that those bodies are the best conductors which contain the greatest quantity, and those the best non-conductors which contain the least.—Thus metallic bodies are the best conductors *. All fluids, except air and oil, are also conductors. The disposition in the body to retain electricity may be termed its capacity.

When conducting bodies undergo any change, if by such change their capacities become altered, then signs of electricity are evinced.

If the change should be of such a nature, that their capacity for electricity becomes increased, the substance will be in a state of abstracting it from surrounding bodies, and therefore will evince negative signs; the same as frigorific mixtures produce negative signs of heat.

If, in the change it undergoes, the capacity of the substance for electricity is diminished, it gives out a portion of its natural quantity, and evinces positive signs, or a state of superabundance.

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* Philosophical Analysis, by C. WILKINSON. Printed for Allen, Paternoster-row.

When any substance, in the change it undergoes, gives out electricity, it becomes proportionately diminished in its conducting powers; so, on the contrary, when it acquires an increase, it increases also its powers as a conductor.

Thus a metallic substance, which is a good conductor, when oxydated is a very imperfect one. In the change from its reguline state to a calx, electricity is given out.

Water, when it forms itself into ice*, gives out electricity; and ACHARD says, that ice reduced to 13° below the zero of FAHRENHEIT, becomes so great a non-conductor, as even to be capable of excitation. Dr. JUST observes, that from a cylinder of ice *he has drawn* sparks when at the temperature of 5° below zero.

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* In order to ascertain the influence of electricity, in the process of crystallization, I made some experiments on the change of water to ice. The temperature of the air in the course of the night was as low as 26° of FAHRENHEIT. It is well known that when water remains quiescent, it may be cooled down to 29° without being frozen. Upon the least motion, it is almost instantaneously converted into a solid mass. Conjecturing this to arise from the absolute necessity of the water to part with an adequate quantity of electricity, as well as of heat, I placed water in two equal sized glasses, and covered their surfaces with oil. Into one I introduced an iron wire, bending its sides over to communicate with the ground; the other did not contain any thing. A SIX's thermometer was placed near them. This instrument indicated the lowest and highest degrees to which the included mercury has been during the night. In the morning it was found at 27°. The water covered with oil was not frozen; the other was completely congealed. At 26° both were frozen.

When water is converted into vapour, its capacity for electricity is increased; and it is well known that the conducting power of vapour is superior to that of water*.

This capacity for electricity is not regulated by any known laws, such as the densities or the specific gravities of the bodies.

In many substances, the conducting power seems to depend on the addition of other principles; thus wood, when a conductor, is so in consequence of the moisture it contains; when deprived of it by drying, it resists the passage of electricity.

Wood, when charred, again becomes a conductor; but the charcoal, when powdered, is a non-conductor. It would hence appear probable, that some change in the arrangement of its own constituent parts, may occasion this variation. Thus a piece of sponge, which, in its compressed state, will retain a very small quantity of water, when in its dilated state, will absorb a considerable quantity of that fluid.

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* This has been fully proved by LULLIN, PRIESTLEY, VOLTA, and LAVOISIER.

What this peculiar change may be is difficult to conceive; but when electric bodies become partial conductors, it seems to be effected by the agency of heat.

Glass, which is a non-conductor, when rubbed becomes a conductor; and is equally so when its temperature is raised to 400° of FAHRENHEIT. The conducting power from attrition appears only to remain while the glass is immediately under the rubbing action. It has but a momentary existence; and the powers of conducting are exhausted, the instant the glass is disengaged from the rubber, no signs of electricity being evinced unless the latter is in contact with a conducting medium. How far this change in the glass may be attributed to an alteration in temperature, appears to merit the attention of electricians. Although no very sensible warmth is induced in the substance of the glass, we are still to learn in what manner its merely superficial particles have been influenced, and raised to a very high temperature. The quantum of electricity thus accumulated, is always in the compound ratio of the surfaces and of the pressure.

When the pressing action is very considerable, as in the case of metallic bodies, great quantities of heat are extricated. Thus a nail, when struck violently, soon exhibits signs of considerable warmth; the caloric diffused in its interstices is exuded on the surface in consequence of the approximation of the constituent particles of the iron. More striking effects relative to the disengagement of heat by violent pressure, are particularized in the ingenious Essays of Count RUMFORD.

Whether the caloric diffused in the interstices, or combined with the body, is given out by pressure, is a fact difficult to determine*. Those substances which are non-conductors,

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* Dr. CRAWFORD supposes heat to be never in such a state of union with matter as to form a chemical combination; but that it is merely mechanically diffused, the particles of fire being disseminated through the interstices of matter, and not in immediate union with the constituent particles.

It is extremely difficult to draw the line of distinction between mechanical diffusion and chemical union; the difference between the highest degree of diffusion and the first degree of chemical combination, is too minute to be in any way ascertainable. In the more simple state of diffusion, in which caloric appears to be disseminated, it does not appear to exist in its elementary state, uncombined with any other substance, but to be combined with some other body, so as to form with it a gaseous principle, the basis of which not being able to permeate the substance, the caloric is evolved, precisely in the same manner as is exhibited by the condensation of air.

The superincumbent pressure on fluid particles effecting in them ductors, and consequently capable, from excitation, of giving out signs of electricity, do not all of them lose their power, when freed from the rubbing action. Those bodies which are usually termed resinous, continue for a certain space of time in their conducting state, until they are equalized with the surrounding air; and, continuing in a disposition to abstract electricity from surrounding bodies, will therefore evince negative signs.

Positive and negative electricities are not to be regarded as two distinct principles; but merely as relative proportions, of the same principle.

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them a change, such as the evolution of a principle similar to the combined caloric, may be considered as the reason why fluids act with a force proportionate to the area of their base, multiplied by their perpendicular height.

Supposing a columnar series of a thousand particles of water. If we conceive that the particle of water in the middle of the column, necessarily compressed, by a force equal to about 500 particles of water, from the superincumbent pressure gives out a certain portion of caloric, the particle at the bottom of the column, pressed upon by a double force, will give out a double quantity. As action and re-action are equal, the re-action of the bottom drop will be exactly equal to the superincumbent pressure; and it will necessarily follow, that all the particles lying in the same line will be equally acted upon and equally re-act. In the finest dust or powder, no change can be induced in the constituent particles by pressure; therefore these particles only act with a force equal to their absolute gravity.

Glass, and all vitreous substances, when excited, give out positive signs of electricity. Du Faye and the Abbé Nollet having observed that ground glass gives out negative signs, subsequent writers have transcribed this error merely upon their authority.

Sealing-wax, resinous bodies, and such of the electrics as are capable of combustion, give out, on excitation, negative signs.

These bodies, when excited, become not only conductors at the moment of excitement, but also retain the power a certain time afterwards, insomuch, that from every substance with which they can come into contact, of a conducting nature, or containing electricity, they will abstract the fluid until they become in a state of equalization. It is for this reason that excited sealing-wax is in a state reverse to glass. The latter gives out the fluid, but the sealing-wax continues to abstract. Hence, when they act together, the sealing-wax readily admitting what the glass is giving out, they counteract each other.

When equal portions of sealing-wax and glass, equally rubbed, be placed both together upon an electroscope, no disturbance of the pith-balls or gold-leaf ensues.

It might be a subject of useful inquiry, whether this power of retention in resinous bodies be in any way dependent on the state requisite for combustibility.

CHAP. XIX.

Different capacities of substances to contain the electric fluid—Caloric one of the component parts of all bodies—Change induced in particular substances by attrition—Influence of the weather and seasons on the result of electrical experiments—Effect of mercurial amalgams explained.

AT present we are unacquainted with any substance which does not contain some portion of electricity; and in general we find, that when any change is induced in a body, so as to increase its capacity for heat, its conducting powers are proportionably augmented.

When no signs of electricity are apparent, it is because all the substances contain precisely such a quantity of the fluid as is proportionate to their capacities.

It is well known, that equal bulks of ice and water, each of them at the temperature of 32, possess, notwithstanding, different capacities for caloric, different quantities of which they contain. Yet, if these two bodies be placed in contact, no changes will be induced, because they are in a state of equalization, in proportion to their capacities. If the temperature of the water should be increased to 42°, viz. ten degrees above the freezing point, and consequently so much greater than the temperature of the ice, the caloric in that part of the water in contact with the ice, meeting in this substance with less resistance, will be pressed forwards, and enter into union with the ice*.

Thus, although ten cubic inches of a metallic substance contain considerably more electricity than the same bulk of water, or of air, yet, while in contact with these substances, no disturbance as to this principle will take place, because their capacities are proportionate to their quantities. But if either of them should undergo any change, so as to influence its capacity, then a disturbance will take place. If, for instance, the water or the air should be expanded by caloric, its capacity for electricity will be increased, and the electricity inherent in the metal, meeting with less resistance in that direction than any other, will

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[*] Si par exemple, on prend une masse de glace, à la temperature 32° de ce thermomètre, et qu'on la mêle à une masse ègale d'eau à 170°, lorsque la glace sera fondue, on aura la masse totale en eau à 32°. De Luc.

consequently quit its residence in the metallic body.

When it is said that one body abstracts electricity from another, it is not to be understood that this is effected by any peculiar power in the body receiving it. On the other hand, it is to be explained by the common principle of all bodies moving in the direction in which they meet with the least resistance.

Thus air, which is disseminated through water, becomes so by the general pressing force. When water, freed from air, is exposed to the atmosphere, it is pressed upon by the aerial mass, and, in consequence of this pressure, a certain quantity of air is forced in, and is merely retained there by the resistance of the surrounding air. If the density of the surrounding air be increased, more air will be forced into the water. As the surrounding air is diminished, it will become diminished in its density, and the air previously forced into the water will now pass out, until the quantity in the water is exactly counteracted by the pressure without. When water is placed in an exhausted receiver, air escapes, because the pressing force which kept the air in the water is now removed.

It is the same with caloric, of which all bodies contain a certain quantity, and this quantity is regulated by the combined circumstances of the disposition to receive it, and the pressing force. This may be illustrated by the example of sulphuric acid and water: supposing equal bulks of these fluids, each of them will contain a greater quantity of caloric than the same bulk of air. The air admitting of a certain quantity only, its resistance to the admission of more, is a precise counterbalance to the water. When the water and the acid are mixed together, the disposition to retain caloric becomes diminished; and although, prior to the admixture, they were in a balance with air, yet afterwards, by this change of disposition, caloric will be evolved, until a balance is again restored.

These are the necessary circumstances arising from a general plenum, all these phenomena being merely dependent upon a change of place*.

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* If the idea of a vacuum should be entertained, what difficulties would attend the explanation of the apparent exhaustion of a pneumatic receiver. Let us suppose that in a receiver containing ten cubic inches of air, the capacity of the exhausting syringe be equal to 10: upon the first exhaustion it must appear evident that the air under the receiver will only be of one half of its original density; and as the quantities taken away at any number of successive terms, will form a geometric series, consequently the whole can never be exhausted. We might thus express it by symbols: Let *a* be the capacity of the syringe,

Thus a ball of glass, or of any metallic substance, may be in contact, without any disturbance,

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ringe, *b* that of the receiver: after every exhaustion, the quantity of air extracted is to the quantity before, as a is to $a+b$.

No one can be led to suppose, that after the first exhaustion, the remaining five cubic inches of air in the receiver will occupy a part only of its capacity, and retain their original density. On the contrary, we well know from the barometrical gauge, that in this state the air is only adequate to the support of half the mercurial column it before supported: if we suppose the rarefied air to occupy the whole space of the receiver, by virtue of some *expansive power*, some *energizing principle of elasticity*, how loosely formed must we deem the constituent particles of air, to suppose in them a capability of being so easily expanded and contracted into spaces so greatly different.

NEWTON, although an advocate for this abstruse doctrine, has thus observed: "*Causas rerum naturalium non plures admitti debere, quàmquæ & vera sint et earum phenomenis explicandis sufficiunt.*" To support such a theory after the manner of a BOYLE or a NEWTON, what an infinite number of causes must we suppose, what a complicated system must we form, so contrary to the simplicity Nature herself adopts!

In every operation of Nature, there is some alteration in bulk in the matter concerned. To suppose the augmentation or diminution effected by such a change in the constituent particles, would be an hypothesis involving more difficulties than those attendant on the occult qualities of the Aristotelian philosophy.

Let us consider the atmosphere, formed of aerial particles, of caloric, electricity, magnetism, light, and other extraneous matter, casually diffused through the air, as being universally ance, as to the electricity, taking place. The glass, from some arrangement in its constituent parts,

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versally full, and upon this principle reflect on the nature of a pneumatic exhaustion.

As air cannot permeate a glass receiver, this fluid may be drawn out. In such a case every exhaustion of air from within, when thrown into the general atmospheric mass, must necessarily occupy a certain space; and, as we have supposed a plenum without, unless an adequate bulk of matter enters into the receiver, the resistance to exhaustion will combat every possible effort.

The matter which enters into the receiver must be something more minute than air, as it must pass into it through such media as preclude the admission of air. It is well known that caloric, electricity, magnetism, and light, will penetrate substances impermeable to air. If therefore an adequate bulk of any of these enters into the receiver, an equal space is left for the air thrown into the general mass.

In pneumatic exhaustions, the thermometrical experiment demonstrates that caloric enters into the receiver, which is as full after the air is withdrawn as before, because, in lieu of air, the same bulk of caloric, &c. passes in. The caloric withinside the receiver being by no means a counteracting pressure to the air without, like water diffused in the vascular ramifications of sponge, passes through on the slightest pressure in every direction.

As the receiver is surrounded by a fluid which cannot enter in, it will be acted upon in proportion to its extent of surface.

Hence may be easily deduced that any portion of air, however small, if of equal density with the surrounding air, will be equal to the atmospheric pressure. Therefore the elasticity of air parts, resists the admission of any of this principle. When, by attrition, a change is induced on its surface, the resistance to the admission of electricity becomes diminished, and will then receive some from the surrounding air or the metallic substance, or any conductor of electricity to which it may be exposed. From the surrounding air it would receive so comparatively small a quantity, as not to evince any electrical signs, if the attrition should take place without the connexion of a conducting body.

When the friction of an insulated rubber is applied to a piece of glass, unless a portion of the rubber is a conductor, no signs of electricity will be manifested. This change induced upon the surface of glass, extends to so small a depth, perhaps not the $\frac{1}{1000}$ part of an inch, that, immediately on the rubber being removed, the electricity it received while under the rubbing action, is given out to the surrounding air, which being also an imperfect conductor, the electricity is not instantaneously communicated, but hovers

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air is equal to the pressure, which is in the compound ratio of the quantum of caloric and aerial particles.

The moment a previously exhausted receiver is exposed to the atmosphere, the air rushes in with a violence in proportion to the surrounding pressure, and, meeting in the point of communication with a less resistance, moves forwards with a velocity adequate to the pressure in every other part.

over the surface of the glass, till it is ultimately removed by successive portions of air.

When a plate of glass is excited by a rubber on one side, pith-balls are no ways agitated by being applied to a part of the glass directly opposite to the rubber. The moment this part is disengaged from the rubber, then the directly opposite portion will disturb the pith-balls, by the state of electricity which is necessarily reverse to itself.

On this principle we may easily explain the reason why a small cylinder or plate, with its proper sized rubber, will not produce the same quantity of electricity as a proper sized rubber on a larger cylinder or plate, although it be so revolved as that equal surfaces shall be rubbed in equal times. Let us suppose the rubbed surface of the smaller cylinder or plate to be thirty inches, and that of the larger sixty inches. Now, in order that the surfaces rubbed in equal times may be equal, there must be two revolutions of the smaller to one of the larger; and thus every inch of one machine will be rubbed twice to every inch of the other. Consequently, the same time will not be allowed to every distinct portion of the rubbed surface to be restored to the state required to admit of the greatest degree of excitement.

If the change be dependent on temperature, when the surface of the glass is small, every part passes so quickly under the rubber, that the difference between the temperature under the rubber and the other parts of the glass becomes less, and hence less quantities of electricity are produced than when there is a larger surface.

It is owing to this, that in dry frosty weather, electrical machines are the most powerfully excited: the quicker the glass, after passing under the rubber, recovers its original non-conducting state, the more perfectly it gives out the electricity it receives in the instant of rubbing.

In dry warm weather, the temperature of the glass becoming greater than in winter, will not allow of equal changes.

Glass, when rubbed, should undergo equal actions in every part: it is therefore necessary that it should be in as homogeneous a state as possible. Those cylinders are the best, which are formed of the metal in the middle of the glass-pit, which is more completely fused: at the glass-houses they generally contrive to have a sufficient quantity in the pit for the week's consumption. In such a case, a cylinder blown in the middle of the week, has frequently been found superior to one blown at the commencement or at the end of the week.

The perfection of the annealing or subsequent baking process, does not so much influence the quality of the cylinder as the above. The latter process bringing the constituent particles of glass into a nearer state of approximation, it is rendered less liable to be broken by any tremulous or vibrating motion.

Glass, in its most perfect non-conducting state, appears to contain a small portion of electricity, from being inferior in its non-conducting power to Venetian talc. According to the foregoing principles, it must possess some electricity.

The advantages derived from the excitation of glass by mercurial amalgams appear to be two-fold. The cushion and glass are not only in more perfect contact, but a metallic substance being brought in immediate contact with the rubbed part, renders the pressing force greater, and allows of a ready transmission of electricity to the glass*.

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* After various trials of different amalgams, I have found the following preparation to be the best: Three ounces of zinc, two ounces of tin, and one pound and a half of mercury; the zinc and tin should be melted and poured upon the mercury, placed either in a mortar or in a thick wooden box well chalked within side, to prevent it being burned by the fused metals, and then well agitated. In this state the amalgam should be preserved; and only so much occasionally prepared with hog's lard as may be required for use. When it is to be applied to the rubber, it should be finely powdered, and sifted through a piece of fine muslin, otherwise the glass will be scratched. Now take a quantity of hog's lard which has been boiled at least

We are in possession of no metal beside mercury, which can be so arranged as to be applied to the cushion. Having applied to a cushion a series of silver leaves, I could in no way procure an excitation similar to that produced by the amalgam; the other metals employed are of no other service, than as they assist the reduction of the mercury, and consequently, the greater the proportion of mercury in the amalgam the better. The black dust which appears upon a cushion after having been used for some time, is the oxyde of mercury, similar to the one produced by simple agitation or trituration: the cushion should always be well cleaned before a second application of amalgam is made.

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least half an hour, in order to throw off any water it may contain, and mix the smallest proportion possible of this with the powdered amalgam, so as to enable it to be spread upon the cushion. By these means an excellent amalgam is procured, which will require a considerable time before it will become oxydated. When this happens, black spots will be perceived on the glass; and in such a case the cushion should be well cleaned before any fresh amalgam is applied. If it should be carelessly applied over the old amalgam, the fresh application will soon be destroyed.

CHAP. XX.

The effects of electricity on air—Principles of electroscopes and electrometers explained.

AIR, when very dry, and its sensible heat very low, becomes a good non-conductor. It is not, however, to be deemed destitute of electrical matter * in this state, which merely bestows on

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* Air, when condensed in an insulated ball, gives out electricity in the first operation, and afterwards caloric: air, when condensed, as soon as disengaged, abstracts electricity from surrounding bodies. In an air-gun well charged, if a portion of air be discharged in a dark room, the light disengaged by the sudden abstraction of electricity, renders it luminous.

Air, in expanding, abstracts electricity from surrounding bodies, and consequently the sides of mountains, in giving out electricity to the surrounding air, evince positive signs; thus SAUSSEUR observed, that the higher he ascended, the greater were the signs of electricity he discovered. As ascending air abstracts heat from the mountains, the higher we ascend the colder we experience the air to be. The caloric being given out to the portion of air which is in actual contact, this must occasion a small current, ascending upwards in the direction of the mountain. To this cause was owing the deflexion of the plumb-line Dr. MASKELEYNE experienced at the mountain Scheallien, in on it a maximum of resistance, to the admission of a greater quantity of the fluid. When air becomes warm, or holds in solution any quantity of water, it is then a conductor; and its capacity for electricity increases in proportion to the addition of moisture and warmth. Thus, in very wet weather, it is difficult to perform electrical experiments, there being under these circumstances very little resistance to the equalization of the electrical fluid, insomuch, that any accumulation is prevented.

When the air is dry, the resistance to the electric fluid is in proportion to its density. As action and re-action are equal, so the electrical fluid acts equally on the air, and is generally accumulated to such a degree as to be equal to the resistance of the latter. When it exceeds this point, the resistance is overcome, and the equalization restored: thus air, when condensed, while it opposes a stronger resistance, will also admit of a larger accumulation of electricity. When it is rarefied,

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in Perthshire, and not to any attraction of gravitation, to which Dr. M. attributed it.

Air being rarefied by the caloric it receives from the mountains, rises upwards. The circumambient cooler air pressing towards the mountain, a long pendulous body, surrounded by a medium of unequal resistance, will deviate from the perpendicular line, in that direction where it meets with the least resistance.

rarefied, its resistance is not so powerful, and is overcome by a smaller quantity.

A glass jar of 160 square inches of coated surface was found, when charged in the open air, capable of fusing one inch and an half of lead wire about $\frac{1}{100}$ of an inch thick. When it was placed under the condensing apparatus, it became capable of fusing a greater quantity, in proportion to the increase of aerial pressure. When a jar is placed under an exhausted receiver, the charge is diminished in proportion to the exhaustion.

Were air a conductor, no electrical experiments could be performed, since there could not be any accumulation in the one part, nor deficiency in the other. We should thus be deprived of all the advantages we derive from atmospherical electricity. This principle would not in that case have entered into combination with water, when converted by the solar rays into vapour; and the aqueous fluid, after having passed through its aerial circulation, would no longer have been precipitated in the portions which are so conducive to vegetation. There would have been a dull uniformity in the atmospheric regions; and Nature would thus have been deprived of one of her most energizing agents. On the contrary, we now see that, by this wise provision, action cannot take place, without unfolding a portion of this enlivening principle. The evaporation of a drop of water, as well as the concourse of floating fields of vapour, elicit in their disturbance more or less electricity. To this showers owe their genial power, when they descend to the surface of the earth, and fall on plants which contain different proportions of electricity, the equalization producing the action which stimulates and excites vegetable life.

On the foregoing principle, I shall proceed to an explanation of

ELECTROSCOPES AND ELECTROMETERS.

Electroscopes are instruments constructed of delicate materials, so as to be influenced by very small portions of electricity; for instance, of fine flaxen or silver threads, with or without pith-balls, or slips of gold leaf, as they were first contrived by Bennet. They have been improperly termed electrometers, but are in no respect calculated to measure the intensity of electricity. They merely shew, by their disturbance, the presence of small portions of electricity, as is indeed implied by the word electroscope. For the adoption of this word, we are, I think, indebted to the very ingenious Beccaria.

Electrometers are instruments which in some degree indicate the intensity of electricity, by measuring the striking distance through a plate of air. They are therefore only employed in very sensible accumulations of electricity. The electrometers of LANE, BROOKES, and CUTHBERTSON, are of this kind.

It is well known to electricians, that an excited glass tube, or a stick of sealing-wax, applied near suspended pith-balls, will make them diverge. When they are rendered diverging by the excited glass tube, on the application of an excited resinous body, the separation between the balls will be destroyed, and they will be brought into the state they were in before they were disturbed. If both the excited bodies be applied to the pith-balls at one and the same time, no sensible action will be induced. Their states being different, they counteract each other. DU FAYE, who first observed this, supposed there were two different kinds of electricity, contrary to each other: the one he termed vitreous, and the other resinous. As these states appeared to Dr. FRANKLIN to be entirely owing to the bodies having more or less than their natural quantity of electric fluid, he changed the terms, as before observed, to positive and negative.

The separation of the pith-balls, either from positive or negative electricity, is occasioned by the re-action of the portion of air between the balls, being greater than the one which is produced by any portion of air surrounding the balls in any other direction.

The action and re-action of air and electricity are evident in a variety of instances. Thus, in the experiment of the electrical fly, the electricity pouring forth from the points, in order to equalize itself, meets with a resistance in the surrounding air; and the air, re-acting on the electricity, produces the retrograde motion of the fly. When the fly is placed in an exhausted receiver, no such motion is induced.

If the rods of the universal discharger be arranged with a jar while it is charged, and if, between these rods, a lighted taper be placed, the current of electricity flows from the positive to the negative, and in that direction the flame is blown.

If a candle be held to a ball of a conductor, positively charged, it will not be sensibly warmed; but if it be applied in a similar way to a negative ball, it will be considerably heated*.

If a light, suspended, elastic body, for instance, a glass bubble, *b* (vide *Fig.* 1, *Plate* III.) be placed near an excited conductor M, although the light body be an electric, yet it will be thrown into motion by the re-action of the surrounding air, and consequently impelled towards the conductor M. It is at first observed to move in the nearest direction, suppose *f*; and as, in consequence of its being a pendulous body, it cannot strike upon a sphere in a perpendicular direction, it will therefore be reflected in an angle equal to the incident angle, and will necessarily describe a series of curves.

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* This experiment was communicated to me by the ingenious Mr. CUTHBERTSON.

When an excited glass tube is applied to an electroscope, (vide *Fig.* 2, *Plate* III.), the pith-balls will be influenced, and will participate of the positive state. In their quiescent state, they were in a precise balance with the surrounding air; but in consequence of this increase, they will give out the electric fluid to the surrounding air, until an equalization is restored. When the air is very dry, the communication is, on account of its resistance, so slow, that the pith-balls will continue a long time separated.

The pith-balls, when in a state of surcharge as to the surrounding air, will give it out in every direction, the air radiating in every direction around the balls.

Let *a*, *b*, (*Fig.* 2), represent the two pith-balls, and the dotted lines the radiating direction of the electricity passing from the balls to the surrounding air. Let *e f* represent the portion or plate of air between the two balls. From what has been previously remarked, the re-action of air to electricity has been manifested; and the portion of air between the two balls must in this case remain stationary, from being acted upon by equal portions of electricity on the opposing sides.

Supposing the electricity which emanates from a pith-ball to act upon the plate of air *e f*, or *o p*, with a force equal to 4, as action and re-action are equal, this will be a measure of the re-action of the air, therefore the sums of the re-action of the plates of air *r t*, *o p*, will be expressed by 4+4; the re-action of the plate of air *e f* with respect to the ball *b*, will be 4+4 viz. its re-action, and the force with which it is impelled by *a*. It will be the same with respect to the ball *a*, which will be re-acted upon by a force equal to 4+4; the exterior resistance to the two balls will be equal to 8, while the resistance from the plate between will be equal to 16, as the balls will necessarily move in the direction in which they meet with the least resistance, they will diverge*.

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* The result will be the same, if we suppose that the contrary actions *a b*, on the plate of air *c f*, destroy each other. The re-action will then be equal to 8. But the re-actions of the plates of air *o p*, *r t*, will be each counteracted by the electricity from the balls, and therefore the balls will diverge with a power in proportion to the intensity of the electricity. On this account BECCARIA proposed the contrivance in question, as an accurate measure of small portions of electricity.

When an excited resinous body is applied, it is, with respect to its electricity, in a state of deficiency, and consequently the electricity, in conducting bodies with which it is in contact, will flow towards it in order to equalize itself. The pith-balls communicating with the resinous body, will lose a portion of their natural quantity of electricity, and consequently will be in a negative state.

In this case the pith-balls possess a less portion of electricity than is necessary to balance the electricity of the surrounding air. In the equalization, electricity will pass from the surrounding air, in converging streams towards the balls, so that the dotted lines in *Fig*. 2, will express the direction in this case as well as in the former.

In this case, the re-action must be precisely the same as before; and, whether electricity passes from the balls or towards the balls, the resistance will be exactly the same. Whatever may be its tendency to pass from the air to the pith-balls, the re-action will be in proportion to the resistance it meets with in its passage. Every particle of electricity which is given out will re-act on the air, the same as a cannon ball produces the re-action or the recoil of the cannon.

In this case, as in the former, it must appear evident that the air placed between the two pith-balls will have double the recoiling or re-acting

power

power to what the air will have in any other direction, and consequently that, upon these principles, the balls will diverge.

The electrometer of CUTHBERTSON unites all the advantages of HENLEY's quadrant electrometer, LANE's electrometer, and BROOKES's*.

In *Fig*. 3, a representation of this may be seen. The balls C D are connected with the inside of the jar, and B with the outside; A E C is a balance moving on the centre E; F is the quadrant electrometer, which determines, by its divergency, whether the jar and the prime conductor are of equal intensity.

In the ball C there is a hole to let in the pin *i*, by which means the end C is kept close to D, or else a ring of a known weight slides on the rod E C, and its distance from the centre E, becomes a measurer of the intensity.

When a jar is charged, the ball C rises upwards, and the ball A strikes B, which, communicating with the outside of the jar, produces the discharge.

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* A more minute description of this may be seen in a very ingenious Dissertation of Mr. CUTHBERTSON, in Mr. NICHOLSON's valuable Journal, vol. ii. p. 526.

A very ingenious philosopher, Mr. LAWSON, has made considerable improvements in BROOKES's electrometer, which, as well as CUTHBERTSON's, discharges batteries by its own action. For further particulars, reference may be had to Mr. TILLOCH's excellent Philosophical Magazine, vol. ii. p. 251.

When the jar is charged, Mr. CUTHBERTSON observes, that the two balls D and C repel each other, insomuch that the descent of A is occasioned by the repulsion between D and C.

With due deference to the opinion of this eminent electrician, the explanation given does not appear to me satisfactory: we are only told, that when the two balls contain a surcharge of electricity, they separate; but the cause of the separation is not pointed out. If the ascension of the ball C were to depend solely on the repulsion of the ball D, it must appear evident that the distance between A and B would occasion no difference.

It is said that B attracts A, because B is connected with the negative side of the jar, and is a contrary state to A.

These terms of attraction and repulsion merely express the state of action, but do not explain any of the phenomena. Into such difficulties we are necessarily led, when we attribute certain powers to inanimate matter, by supposing its capacity to act beyond the point where it rarely exists.

I am persuaded that the explanation of this instrument will be attended with no difficulty, upon the principles already laid down.

From what has been observed, it will be evident that the balance AEC is positive with respect to electricity. The ball C, connected with the ball D, as well as with the inside of the jar, cannot unload itself of any of its electricity. The ball A participates of the same intensity. Being in a more balanced state than the surrounding air, it acts upon that fluid, and is re-acted on by it. The portion of air which lies between A and B, being acted upon in the same manner, in order to receive the electricity from A, gives out a portion to B; and in consequence of the air thus unloading itself, the action of the electricity from A upon it becomes less. The re-action in that direction being less than in any other, the ball A becomes pressed on all sides, but least so in the direction AB; A will therefore descend towards B.

If the intensity be measured by the distance between A and B, or by an increase of weight on the other arm EC, it will be found to depend upon the same principles.

The gold-leaf electroscope of Mr. BENNET possesses great sensibility; and, by the improvement of Mr. PEPYS, whose scientific acquirements are so well known, by which the coatings are rendered moveable, very small portions of electricity are discernable. The instrument is so delicately constructed, as to be readily destroyed. Even in drying the outside of the glass, so great a disturbance of the fluid will be occasioned, that the leaves will be determined to the glass, and adhere so as not to be removed without being torn. An extremely fine silver thread, prepared after the manner of Mr. READ, or the minutest thread that can be drawn out of a bundle of very fine flax, with a little isinglass glue applied gently over it with the finger and thumb, when cut into proper sizes, forms a very excellent electroscope.

CHAP. XXI.

The influence of points on electricity explained—Different states of electricity exhibited by an insulated conductor—The electrophorus explained.

MR. HOPKINSON, one of the Pennsylvanian experimentalists, having conjectured that, by the means of points, he could concentrate the electric fluid, and thus produce a more powerful spark, was surprized to find little or no effect produced. Dr. FRANKLIN, who at first supposed that this might depend on the attraction being in proportion to the surfaces, at length confessed, with his wonted ingenuousness, that although this explanation was the best he could offer, he did not think it satisfactory.

VOLTA and other electricians, have supposed points to be coatings to an infinitely small plate of air. Upon the preceding principles the theory of points admits of an easy solution.

Any conducting substance in a positive state of electricity, will act as a centre to the circumambient aerial portions which will recede in concentric circles. A conductor whose surface is large, will require a power to disturb or displace its own electricity, in proportion to its extent of surface.

Let B *(Fig. 4, Plate* III.*)* represent a charged conductor, and C the surrounding electrical atmosphere. If the brass ball E be applied at *m*, it will just be in contact with the electrical atmosphere, touching it merely in a point. Now, suppose the three dots 1, 2, 3, to represent three contiguous particles of electricity; and the particle 2, to be the only one acted upon by the electrical atmosphere: in this case the resistance of the particle 2 to any motion, will be greater than the action of a particle of electricity in the confines of the electrical atmosphere; seeing that the above particle 2, when disturbed, will itself act equally all round, and will therefore disturb the contiguous particles 1, 3, &c. To enable the electricity of the ball to communicate a spark from the conductor, it must be placed within the atmosphere, as at *n*.

If a pointed body D, be placed just within the verge of the electrical atmosphere, as at O, and if we suppose 1, 2, 3, 4, 5, 6, to represent the direction of the electricity; at the extreme point O only a solitary particle will exist, and the remaining portion will be in a right line with the direction of the impelling power. In this case the resistance will be overcome as soon as the point enters into the verge of the circumference of the sphere formed round the conducting body: one of the particles being removed, another will succeed, and thus the point will silently steal away the fluid.

The conductor B, when charged, possessing more electricity than the surrounding air, will give out a quantity of it, until an equilibrium is restored. The density of the electricity will be the greatest in the part contiguous to the surface of the conductor; and in receding from it, its intensity will be not only diminished, but also weakened in its action, by the increasing resistance of the surrounding air.

To give motion to the natural electricity of the brass ball, it must be placed sufficiently within the sphere of electricity surrounding the conductor, as to be enabled to acquire a due degree of intensity; and the larger the ball, the nearer it must be stationed to the conductor. It is well known, that when a long spark is required, a small ball is employed; but that, if the spark is to be dense, a large ball is necessary.

A point, comparatively speaking, has at its extremity the resistance only of a solitary particle to be overcome. If, therefore, it be placed in the verge of the circumference of the electrical atmosphere, it will slowly and silently steal away the fluid. The ingenious BECCARIA has observed, that two needles, exposed to a charged jar, would free it of its electricity in half the time that would be required by a single needle.

As the density of the spark is diminished in the inverted proportion of the surface exposed, a point only exhibits a kind of luminous star just at its extremity.

A pointed body being projected from the conductor, the electricity likewise passes with an increased facility to the surrounding atmosphere, and exhibits a ramified appearance in the form of a brush of light.

I shall now consider the different states of electricity exhibited by an insulated conductor, when one of its extremities is placed within the influence of the prime conductor.

Supposing AB, CD, *(Fig. 5)*, to be two metallic cylinders, placed in a right line with each other, and insulated; then AB, which will constitute a part of the prime conductor of the machine, will, upon excitation, be in a state of surcharge with the surrounding air. If the insulated conductor CD be placed in the same right line as AB, and the end C brought within the electrical atmosphere of the end B, a disturbance will take place in the natural electricity of the conductor CD, the end C exhibiting negative signs of electricity, and the end D positive signs.

If the conductors were to be in contact, there would then not be any unequal portions in CD.

The surcharge in A B would equalize itself with C D; when the connexion is destroyed by the intervening and surrounding air, the ingress of electricity to C D becomes proportionately resisted. In this case the aerial portion between B and C, being acted on by the surcharge of electricity from B, parts with a portion of its own electricity to C, meeting in that direction with less resistance. The air being thus freed of a portion of its own electricity, and receiving it at the same instant from B, the natural electricity of the conductor is impelled in the direction from C to D, the impelling force being in proportion to the intensity on A B.

Presuming this to be a very important part of electricity, upon which many phenomena of galvanism seem to depend, I shall endeavour to illustrate my meaning, by substituting water for electricity.

Let us suppose that AB CD *(Fig. 6)*, is a cylinder containing water in the portions AB, CD; and that there is not any direct communication between the water contained in each of the portions, which are separated by means of the pistons *n*, *o*, connected together by the rod *m*. Now, let *a*, *b*, *c*, *d*, be small pipes communicating into the cylinder C D. Neither of the cylinders is to be supposed full, or to have its maximum of charge. The pistons *n*, and *o*, being pressed upon by equal quantities of water, will remain stationary. If an additional quantity of water be forced into the cylinder AB, its action upon the piston *n* will drive it forward, as it will also the piston *o*, to which it is connected, towards *p*. The water in the cylinder CD being thus impelled, will pass out of the tubes *c*, *d*, while the tubes *a*, *b*, will readily receive more water than before: *a*, *b*, are negative, and *c*, *d*, positive, with respect to water. By substituting electricity for water, and considering the space between the pistons *n*, *o*, as the intervening plate of air, the reason will be evident why the end C should exhibit negative signs of electricity, and the end D positive signs. Electricity, to be disturbed, requires a certain power. It is not moved without some resistance, and when once thrown into motion, possesses no powers in itself to return again to its pristine state. It will therefore, for a certain period, remain in a state of accumulation; or rather retain its tendency to persevere in the motion given. Let us suppose a particle of water to be driven forward in a capillary tube. If, after it, a succession of watery particles are thus impelled, an accumulation of motion is occasioned in the former, which may be compared to that induced on pendulous bodies. If a heavy pendulum requires a certain power to put it into motion, and if, at equal returns, the same power be applied, the pendulum will describe larger and larger arches, retaining the original impulsion, and accumulating all the subsequent motions.

Some idea of the motion induced on the electricity contained in the surrounding air, may be formed from the circumstance of the disturbance produced in an electroscope by the remote application of an excited glass rod. The latter, when at the distance of five or six feet from a sensible electroscope, will occasion a divergence. Upon the removal of the rod to a greater distance, the gold leaves, or pith-balls, do not retain any state of electricity, but return to their natural state. By moving the rod backwards and forwards, the balls play accordingly, diverging and converging. As yet, however, the disturbance induced on the electricity in the surrounding air, communicated more sensibly through the electroscope, only occasions a wave-like motion in the electric fluid, without producing any permanent change, which cannot take place until the glass rod be brought so near as to have imparted a portion of its superfluous quantity. This experiment evinces the facility with which the electricity in the surrounding air is disturbed. Consequently any portion of air, under the influence of an electrical charge, will easily part with its natural electricity, if in contact with any good conducting substance.

I shall now apply the above principles to the explanation of

THE ELECTROPHORUS.

This curious and simple instrument was invented by the celebrated VOLTA, Professor at Como. It consists of a round metallic plate, called by VOLTA, *scudo*, and a plate covered with a resinous substance. The scudo has an insulating handle. When the resinous cake is slightly rubbed by means of a hare or rabbit-skin, and the scudo applied to it, connected at the same time by any conducting substance with the ground, upon suddenly breaking the connexion, and raising the scudo by the insulating handle, several curious effects will arise. 1st, A spark will strike to any conducting body brought near the scudo. 2d, The scudo is always in an opposite state to that of the excited plate. 3d, These effects are repeated without any fresh excitations.

This forms a very simple, and almost perpetual electrical machine, in very dry, frosty weather. When once excited, the excitation will continue very sensibly for the space of two or three weeks. Upon this principle LICHTENBURG constructed an electrophorus of an oval form, about 7 feet by 9. All the excitation required, consists in gently rubbing it with a fox's tail; the metallic scudo, when of this magnitude, is raised up by pullies, and the striking distance is so great, as to pass through 18 inches of air, the intensity being at the same time such as to possess all the powers of a large battery.

The composition employed by LICHTENBURG for his resinous cake, consists of 40 parts of resin, 4 parts of yellow wax, and 1 part of lamp-black.

Mr. NICHOLSON, in his justly-esteemed Journal, conjectures the action of the electrophorus to depend on the compensation of the two electricities. He supposes the electricity to be generated by the excitation of the electric, and to exist there in a state of superfluousness, insomuch that what is given out by the scudo, is what has been received from the electric. It is with extreme diffidence that I presume to differ in opinion with the above gentleman, for whose superior talents, and well-established philosophical reputation, no one can entertain a higher respect than myself*.

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* Dr. YOUNG, in his Philosophical Syllabus, says, that the electrophorus is employed to induce a state of electricity in another

It appears to me that the explanation of the electrophorus is very simple, upon the principles I have thus advanced; and as the application of them to every electrical phenomenon is so very easy, I should hope it is a presumptive proof of their probability.

When a resinous substance is excited, negative

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another body. The partial redundancy or deficiency of the second body being communicated to a reservoir, a temporary equilibrium is produced, which is succeeded respectively by a deficiency or redundancy when the two bodies are separated. This may be easily supplied from the earth, and the operation may be continued at pleasure. If I understand Dr. Young right, by the second body he means the scudo; if so, the term *partial redundancy* or *deficiency* applied to it, does not appear to be correct, the charge being induced on the electrophorus, and not on the metallic plate. An objection may also be made to Dr. Young's ideas relative to the condenser of electricity. A plate of air being interposed between the two metals, when the electricity of one of them becomes disturbed, and determined in any particular direction, the repeated application of small quantities of electricity, occasions a change in the air similar to that of a Leyden phial; and, as happens in the case of the latter, one side cannot give out any signs of electricity, unless the other be equally affected. It is not until the separation of the plates, that the accumulated mass produces evident disturbances of the electroscope.

What Dr. Young means by the excitation of electricity by friction, aided by chemical action, I do not understand.

signs of electricity are always evinced. The excitement having increased its capacity for electricity, it readily receives it from all those substances which are conductors. When the scudo is applied, it can only, while in an insulated state, part with a small quantity of the fluid; but when it is connected with a conductor communicating with the earth, such as the finger of the experimentalist applied to the electrophorus, and removed just before the scudo is elevated, then the electrical appearances occur as above particularized.

The resinous plate, in consequence of the excitement being thrown into a state adapted to the reception of electricity, in proportion to the change it has undergone, will abstract the electric fluid from the scudo, and from the other bodies with which the latter is connected.

The metallic plate, when in contact with the surrounding air, would remain undisturbed, if the two electricities were in a balanced state; but if the surrounding air should acquire an increase of electricity, without its capacity being increased, it will impart to the metal what will be necessary to preserve the balance.

Thus the metallic plate, being in contact with the electrophorus before excitement, no changes are induced, their respective capacities and the natural quantities* of electricity being precisely proportionate; but if the capacity of the electrophorus be increased, a current of electricity will be produced, which will be in the greatest intensity in the part of the scudo in contact with the plate. When the communication is suddenly broken, and the scudo elevated by the insulating handle, then the scudo possesses more electricity than the surrounding air to which it is now exposed,

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* It is not to be understood that they possess equal quantities, it having been already observed that electrics, as non-conductors, contain very little of the fluid; and therefore their resistance to the admission of any more, to be added to the little they may possess, is upon a precise balance with the large quantities of electricity contained in good conductors, added to their smaller resistance. This might be illustrated by a simple mechanical experiment, that of rendering the momenta of two unequal sized balls equal. A ball weighing one pound, moving ten times as fast as a ball of ten pounds, the two balls will, if in opposing directions in their concourse, entirely destroy the motion of each other, seeing that what the smaller ball wants in matter, it makes up in velocity. In the same way the minute quantity of electricity contained in electrics, added to the great resistance of the substance to the admission of more, becomes a precise balance to the great quantity in good conductors. As electricity moves in the direction in which it meets with the least resistance, if, by attrition, the capacity of the resinous substance becomes increased for electricity, or if the state of the arrangement which resisted the admission of more electricity becomes altered, in this direction the electricity from surrounding bodies will necessarily proceed.

posed, and will exhibit electrical phenomena in proportion to its intensity.

It must appear evident that the scudo in this case will exhibit positive signs of electricity, while the electric cake will occasion the balls to diverge with negative electricity. When the pith-balls are made to diverge by the application of the scudo, they will, when in this state, be made to converge on the application of the electric plate, which will necessarily occasion opposite degrees of electricity.

Mr. Bewley, and my respected friend Mr. Reid, have communicated very ingenious papers upon the electrophorus. What proceeds from the latter gentleman upon electricity must be ever deemed valuable.

CHAP. XXII.

Theory of the Leyden phial—Of spontaneous discharges—Uses and advantages of the metallic coatings explained—Theory of the different electric phenomena.

THE charging of a Leyden phial is occasioned by the imperfections of the conducting power of air, and the action and re-action existing between electricity and air.

The glass* answers no other purpose than that of furnishing a more perfect resistance to the communication between the plates of air contiguous

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* It has been asserted by some philosophers, that glass is permeable by electricity; and the Abbé Nollet fancied he had demonstrated this by cementing a jar with a receiver, and making the electricity pass through during the exhaustion. This circumstance Beccaria has very satisfactorily explained. He also observes that white glass, when recently blown, is not so imperfect a conductor as it becomes afterwards. Dr. Priestley has also noticed, that a piece of gold-leaf, being tied between two glass tubes, one old and the other new, on sending a powerful charge through, the metallic sparks were more indelibly impressed on the new tube than on the old. Mr. Canton had remarked that Florence flasks cannot be charged, owing to some unvitrifiable portions they contain. The attention of electricians

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tricians has been excited by the experiment of Mr. Wood of Cambridge, by which, it is said, the permeability of glass is evinced. Mr. Wood placed an electroscope on a glass plate, and covered the former with a glass receiver, in such a way as that it was in every direction surrounded by glass. In this state an excited glass rod, applied at some distance from the receiver, influenced the electroscope to such a degree, as to make it appear to Mr. Wood that this could not be effected without the electricity permeating the glass. Upon the first point of view such might appear a natural conclusion; but, upon varying the experiment, the error into which Mr. Wood fell will appear evident. If the electroscope be placed upon a table, and not upon a glass plate, and then covered with the receiver, no disturbance will take place; and when the apparatus is arranged after the manner of Mr. Wood, the nearer the excited glass rod is brought to the part of the glass plate on which the electroscope stands, the greater will be the disturbance. It must appear evident, that the electrical communication takes place at the part of the plate of glass on which the receiver stood. In the case where the plate of glass should not be employed, the air under the receiver would not be disturbed; and the communication to the earth would be formed in an immediate way by the table. This apparent permeability therefore arises from the extended insulation of the receiver.

This has been tried in another manner.—An electroscope having been placed withinside a glass, which was afterwards hermetically sealed, a disturbance was, notwithstanding, evident on the application of the excited tube. It is a curious circumstance, tiguous to its surface, and thus enabling greater changes to be induced.

When a jar is charged, if it were possible that the glass should be suddenly annihilated, the same electrical appearances would be manifested as before. The mode in which electricity acts upon air has been frequently proved; and this action is necessarily in proportion to its intensity.

Let M S represent a Leyden phial *(Fig. 6, Plate IV.)* insulated on a stand Q. Let it be coated to *a* on the inside, and *b* on the outside, and let N M express a single column of air and electricity, as it naturally exists on the inside, and M O a similar column on the outside. Now, let us imagine the triangles to represent particles of electricity,

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cumstance, that glass, when once in a state of separation, cannot, by the usual process of sealing, by which it is softened so as to be capable of being drawn out, be brought into so perfect a state of union, but that electricity will be enabled to pass through. If the glass be brought into a state of fusion, then the passage of electricity is completely prevented. To this cause is owing the curious phenomenon of the stream of light observed in the empty part of a thermometrical tube, when the bulb is exposed to sparks from the conductor. In this case the hermetical sealing is simply affected by the softening of the glass. That the experiment made by Mr. Wood depends upon the charge of air, is evident. The portion contiguous to the surface of the glass being charged, when the excited tube is removed, and the electroscope in a quiescent state, on applying the hand to the glass, a disturbance again takes place.

tricity, and the circles particles of air. These are merely assumptions for the sake of illustration.

Let O R be a wire, or any other conducting substance, which may be applied occasionally to the outside coating *b* O; and let P be the conducting substance, connected with the prime conductor of an electric machine.

In their natural state, the column M N is an exact counterpoise to the column M O. Both the inside and outside of the jar, as well as all the space, being absolutely full, an additional particle of electricity cannot be forced into the jar, insomuch as it is necessary that it should occupy space. In consequence of its being attempted to be pressed in, it will act equally all around; and, if resisted equally in every part, the admission of an additional particle becomes impracticable.

From the construction of the Leyden jar, this is not the case, since, on the admission of particles of electricity, 1, 2, 3, 4, for instance, the resistance is less in the direction of N M O than in any other, and therefore a charge will take place.

This is occasioned by the column M O parting with its electricity at the point O, which communicates with the ground, through the medium of the conductor O R: if there should be no conducting medium, and the jar should be insulated on the glass pillar Q, then the resistance in that direction, as well as in every other, will prevent any accumulation of electricity within the jar.

It may be difficult to conceive, on a cursory view of the subject, how the addition of any electrical matter withinside the jar, should occasion an abstraction from the column of air on the outside. This may perhaps be better understood by the following diagram, in which other fluids are substituted, to the end, that the effects may come more immediately under our senses.

Let A B C D (*Fig.* 8, *Plate* IV.) represent a double glass syphon, the end A terminating in a vessel filled with charcoal dust. Now, supposing the glass tube to be filled with water and oil, the water will pervade the charcoal powder, but this will not be the case with the oil. If an additional quantity of water be forced into the tube at the end D, both the oil and water will be pressed forwards by the force exerted at D. It is evident, that if at A there were no exit for any portion, every effort to force in more at that point would be fruitless, as the charcoal powder at E is capable of conducting water, but not oil. The water in the tube A B C will be gradually separated from the oil, and pass through the vessel of charcoal E out at G. If the water be still forced in at D until it fills the tube to M, DCM will be filled with water, and ABM filled with oil.

The application of this diagram to the Leyden phial, is, I presume, very easy. The part D of the tube corresponds to the wire connected with the prime conductor; CMB to a column of air and electricity on the inside of the jar; B to a similar column on the outside; and E, the charcoal reservoir, to the earth. By substituting electricity for water, and air for oil, the same circumstances will precisely take place. If CBA be supposed a column of air and electricity; if any additional quantity of electricity be forced in at C; and if at A, the air can unload itself of its natural quantity of electricity, through any conducting medium to the earth, by continuing the process, we shall have the column AN to consist of air freed from its electricity, while the column CM will be formed of electricity freed from air.

As the column unloads itself at the point A, the resistance there is the least. The greater the distance from A, the greater is the resistance, which at NBM is the greatest. If the column NA, be greater than MC, the resistance will increase in proportion, until the intensity of electricity in the prime conductor will lose its influence and ascendancy, and then a charge will become impossible.

M. Cavallo has observed, that a thin glass will receive the greatest charge; but this is not correct. When we want to charge a small jar powerfully, we select a thick one. A thin jar, it is true, is more easily charged, but if the intensity of the machine be considerable, the accumulated electricity withinside the jar, acting equally all around, the same on the glass as on the air, the glass will break.

Spontaneous discharges only occur when the air about the sides of the jar is very dry. So disposed is the surface of glass to moisture, that, in every instance, the portion of air which is in immediate contact with the glass, affords a less resistance than the other portions, in consequence of its proximity to a surface possessing the above cited quality*.

When the other portions of air are very dry, the resistance being less in the direction NAM *Fig.* 7, than towards the interior of the glass jar, the charge goes on so rapidly, that the resistance *d* MC is overcome, and the equalization restored.

If we suppose, in the charged state, N*d* to represent the column of electricity, without air; *c*O the column of air without electricity, or at least gradually freed from electricity towards O;

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[*] Glass, in its driest state, possesses a film of humidity. When beyond the verge of the coating, it meets with less resistance in that direction than in any other.

and cMd the air and the electricity in their natural state; in this case cMd is the resistance which prevents the two columns from equalizing themselves. If the column Nd be increased towards 1, the column cO will also be increased towards 2, and the resistance to the equalization will be measured by 2 M 1. If this should be no more than the striking distance of the machine, it will be broken through, and there will be a spontaneous discharge. This is the direction which the spontaneous discharge always takes. It generally leaves a marked line on the surface of the glass from M towards 2, as if it brought about the de-oxydation of a portion of the lead in the composition of the glass.

When, in dry weather, a jar is thus spontaneously discharged, it prevents the accumulation which is sometimes requisite. We are indebted to Mr. CUTHBERTSON for a mode of preventing this, by simply breathing into the jar. About ten or eleven years ago Mr. C. was led to this observation by noticing that jars, when fresh coated in dry weather, admitted of a greater charge than those which had been coated some time before. The difference appearing to him to arise from dampness, he found, that by breathing into a jar through a glass tube, it was enabled to retain double the usual quantity of electric fluid.

Mr. NICHOLSON supposes, that the surface of clean glass may become charged in successive zones of the contrary electricities, and that these zones, when they have acquired the requisite intensity, may explode into each other, and produce an undulation in the whole charge, which may favour its flight through the space or interval between coating to coating. When the surface is covered with distinct insulated particles of moisture, the escape from particle to particle must be by smaller leaps, and the charge will be more tranquil.

This explanation is attended with many difficulties, it being altogether hypothetical, to suppose that the inside of a jar contains on its surface different states of electricity. The metallic coating is a conductor superior to water, on which account no undulation can arise; and the higher the coating, the more frequent must be the spontaneous discharges.

Mr. CUTHBERTSON supposes the breathing to occasion a film of humidity over the uncoated part of the jar, and accordingly conjectures this to be the reason of the increased charge.

If this were the case, it ought to be overcome by the continuance of the metallic coating to the mouth of the jar; but this is well known to occasion an earlier spontaneous discharge. Mr. CUTHBERTSON observes, that water not being so good a conductor, as any given metal, the spontaneous discharge will not take place in a humid coating. If the uncoated part of the inside of a jar be moistened, we do not experience the effects we ought to expect from Mr. C.'s reasoning, since, by thus diminishing the resistance, we occasion a quicker spontaneous discharge.

When we breathe into a jar, we direct our tube to the air about the coated surface of the jar, by which means we render the air withinside the jar more humid. It being in this state a better conductor of electricity, the resistance to the interior part is consequently so diminished, as to allow of a great charge of electricity. That the resistance to the passage of electricity is by this process diminished, is evident from there being no corruscations or luminous flashes; while, in the dry state of the jar, the inside, a little before a spontaneous discharge takes place, is filled with lambent streams of light.

When a jar having a narrow neck is charged, and then reversed with its neck in water, in proportion as it gradually becomes uncharged, the electricity is conducted by the water; and, as a proof that it occupied space, the water itself will rise up into the jar*.

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* All the phenomena of atmospherical electricity depend upon the different aerial portions. As the capacity of the air for

Let me now proceed to an explanation of the

METALLIC COATINGS.

Atmospheric air being a very imperfect conductor, in order to charge the portion which is

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for electricity varies with its temperature, a change will take place as to its electricity, which, in equalizing itself, will displace the intervening portions of air; and thus the phenomena of thunder and lightning will ensue. Immediately after the report a wind is always observed moving from the negative to the positive cloud. A cloud, when highly charged with electricity, contains less in bulk of aerial particles than another cloud not so charged, because part of its space is occupied by electricity. A cloud weakly charged, and containing a greater proportion of air, in its equalization parts with the air, and receives electricity; consequently the current of air must always be reverse to the direction of the lightning.

It rarely happens that in this equalization, the earth is in any degree concerned. When, however, the lightning strikes upon the earth, this is merely occasioned by the earth acting as a conductor between two masses of air; and this happens in those situations only, in which the distance between two electric clouds is greater than the sum of the distances from those two clouds to the earth.

In the tropical regions the north-east and south-west trade-winds are continually determining masses of cool air into those warmer latitudes. Caloric is, as well as electricity, abstracted from the tropical air; and thus are occasioned those universal electrical diffusions which give the sky the appearance of being covered in every direction with one continued sheet of lightning. These phenomena generally take place so high up in the air, as not to occasion much danger on the surface of the earth.

contiguous to the surface of a glass jar, it would be necessary that the conducting wire should be moved round to every part. By spreading over the surface of the glass a metallic coating, which presents, as it were, a point to every portion of the plate of air contiguous to the surface of the jar, a communication is speedily formed.

So considerable would be the resistance, that it would be impossible to charge a dry and uncoated jar. But if a jar be coated withinside, the resistance of the air on the outside, while it is charging, is evident from the flashes of light given out.

The coating answers the purpose of a communicating medium to the surrounding air, in order that a charge and a discharge may be effected with the least resistance.

The charge of a jar neither exists in the coating, nor in the substance of the glass, but merely in that stratum of air which is contiguous to it. If the coatings be made moveable, and the glass separated from them when charged, by substituting in their place different metallic coatings, the same discharge will take place.

A jar being charged and insulated, if the hand be introduced to the inside, without any communication being established with the outside, no discharge will ensue, neither will the charge be diminished. The columns of electricity and air being balanced by their mutual resistances, the action of the air cannot be removed, unless that of the electricity be also taken off.

Some difference is experienced relative to what is employed as the coating medium; and this seems to depend upon the conducting powers. Thus metal is superior to water, because it furnishes less resistance to the facility of communication.

It has been observed, that in the charging of a jar, it is necessary that the external and internal columns should correspond. If the column on the external surface should extend beyond a certain limit, the column on the inside will require an additional power to overcome the resistance, and will, in a proportionate degree, abstract from the intensity of the machine.

It is on this principle that the maximum of charge, as to the coating, is, when there is an exact correspondence between the metallic coatings stationed in an opposite direction to each other.

Let AB, *Fig.* 10, represent a section of a Leyden pane of glass. Now, if *a b* be the coated surface on one part or side, then *d c* ought to be the coated surface on the other side, in order to produce the greatest effect.

Let us suppose one side coated, *d c*, and the same extent of surface *b e*, on the other side, but not opposed to it. If *b e* is the charged surface, the column *b* A, is greater than the column A *d*, and the loss sustained in this way will be the quantity of electric fluid equivalent to the resistance of the column *a b*, being the difference between the two columns *b* A and A *d*. But *e* B being less than B *c*, to overcome the greater column, B *c* will require an additional power equal to *b c*, the difference between B *c* and B *e*. When the column, and the spaces on each side the coated surfaces, are equal, there is less resistance to overcome, or less power lost; and these spaces can alone be equal, when the coatings are directly opposite to each other.

When a jar is about half an inch thick, it becomes very difficult to be charged. Supposing A B (*Fig.* 9), to represent the thickness of the jar, it must appear evident that the thickness D C will oppose the same resistance as if a moderate jar were placed on a glass plate of that thickness.

The utility of the coatings is evinced by the remains of electricity in a jar after the first discharge. This is called the residuum. The accumulated electricity in contact with the metallic coatings, is discharged at once; but this is not the case with the portions of air lying on the uncoated surface. Although they do not contain an equal intensity with the former, they still suffice for a second discharge when the air is dry.

The residuum alone of the Haarlem apparatus made by Mr. CUTHBERTSON, melted two feet of wire*.

From these observations, I presume to infer that all the phenomena of electricity depend on the surrounding air, and on the mutual action and re-action which subsist between them. Any such action or re-action is not, however, to be attributed to any peculiar power or influence in electricity or air, except what is common to all other bodies.

Heat, light, and magnetism, are principles only evident to our senses through the effects induced by the different resistances they encounter. If any given space were not to afford the least resistance, the passage from one part to another would be effected with the utmost tranquillity. It is, however, ordained, for the wisest and best of purposes, that from the various resistances these principles meet with, they should occasion those changes which are conducive to our general happiness.

Thus the different resistances which bodies, in their different states, furnish to heat, are productive to us of the greatest advantages. Water, when gradually converted into ice, gives out a great portion of heat, which mitigates, in the animal and vegetable kingdoms, the effects that result from the sudden changes of temperature, and prevents a too rapid congelation of the water. So likewise, in the tropical regions, water raised up into vapour, abstracts a great quantity of heat, and thus diminishes its intensity. At night, it precipitates it in a gentle dew, and by this change of capacity, gives it out again, and thus preserves an uniformity in temperature.

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* V. Description d'une trés grande machine electrique placé dans le Museum de TEYLER, a Haarlem, par MARTINUS VAN MARUM.

Thus the varying state of the atmosphere seems in a great measure to depend on the principle of electricity. VOLTA and REID have observed a coincidence in the daily changes of the barometer, and the state of atmospherical electricity. As the barometer is higher in the morning and evening than in the middle of the day, so likewise has the atmosphere, at these times, been observed to be more electrical.

The variation in the rise of mercury, is the greatest in those latitudes in which the greatest changes in the state of electricity are also to be noticed.

At St. Helena there is little or no variation. At Jamaica rarely more than $\frac{3}{10}$ of an inch. At Naples one inch. In England $2\frac{1}{2}$ inches; and at Petersburgh three inches and one third.

To this change of capacity which each body, in its alteration from one state to another, uniformly undergoes, may be ascribed its peculiar arrangement. All bodies in a fluid state, in passing gradually into a solid form, assume a regular arrangement, peculiarly appropriate to each of them. The symmetry noticeable in crystallization is owing to the influence of electricity; and thus, in the aerial regions, we oftentimes see the clouds assume a fleecy appearance, and shoot out in a crystallized manner, merely through their change of capacity for electricity. To this cause the peculiar formation of hail * is owing; and, in a higher change, the production of snow †.

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* A remarkable instance of very large hailstones which fell in London, May 18, 1680, is recorded in the posthumous works of Dr. HOOKE. Dr. DERHAM observes, that he saw one which measured 13 inches in circumference.

† The protection which snow affords to vegetation, is ascribed by HASSENFRATZ to a quantity of oxygen with which it is combined.

CARRADORI DE PRATO concludes, from a variety of experiments, that the preservative quality of snow acts indirectly as to vegetation, by merely preventing the effects of cold. He found snow-water to be so totally deprived of air, that fishes could not live in it.

BERGMAN remarks, that snow affords an indication of nitrous acid. This may perhaps explain why shoes are so soon destroyed by walking on snow; seeing that, agreeably to this hypothesis, effects similar to those which result from an expo-

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Not only heat will influence the capacities of bodies for electricity, but also light. This is peculiarly observable in the fogs which are so frequent in London, and which rarely commence till an hour after sun-rise. As water cannot be preserved in a vaporific state without electricity, so the disengagement of this principle by light, occasions these cloudy precipitations, which continue till the transparency is restored by its re-dissolution*.

On the influence of the same universal principle, electricity, depends the production of various atmospherical meteors. In the lower regions of the atmosphere, thunder and lightning

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sure to a very diluted solution of nitrous acid, will take place on the leather.

The celebrated Mr. Cavendish has shewn, that azote is the basis of nitrous acid. Through the influence of electricity on a mixture of oxygenic and azotic gases, in the conversion of water, in a vaporific state, into snow, the sudden disengagement of electricity and heat may produce the above acid in the instant of congelation. If so, may not this combination be beneficial to different soils?—Dr. Ingenhousz experienced considerable advantages from the employment of sulphuric acid in agricultural experiments.

* May not the peculiarly offensive smell attendant on extensive fogs, arise from the water being decomposed, and the nascent hydrogen so readily blended with azote, as to produce ammonia?

are produced, by a comparative accumulation or deficiency of electric matter in the floating fields of vapour. In the higher regions, probably in that part of the atmosphere where it just loses its property of reflecting light, and which is easily ascertained to be about 40 or 50* miles high, the little streams of light called shooting stars appear. That mad enthusiast, PARACELSUS, imagined the stars to be alive; that they ate and drank; and that the falling stars were their excretions. Mr. BRYDONE observes, that when he was at the top of Mount Ætna, they appeared to him as high as when he was standing upon the plain. Indeed, in the case of all remote objects, the visual powers are not capable of discriminating the different distances, as the rays proceed parallel from all of them, so, with respect to them, the eye preserves the same formation. Those which are situated in a rare part of the atmosphere, are only visible to us on a clear and fine evening, and appear straight and white.

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* This is ascertained by knowing the number of degrees the sun is below the horizon at the end of twilight; the radius of the earth being also given. The height at which air ceases to reflect light is thus easily ascertained. Supposing the depression of the sun to be 18 degrees, and allowing for the horizontal refraction half a degree, the height, by a simple trigonometrical process, will be 44 miles.

Still higher up, the phenomena called fire-balls appear; but how they are formed, is not at present known. Some persons have ingeniously conjectured, that a supernatant stratum of hydrogen gas, of diminished density, may float on the atmosphere, like æther on water. If, this being pre-supposed, in the line of union of these two airs, a quantity of electricity should be disengaged, combustion would take place, and would be communicated instantaneously, as it were, to the succeeding portions, so as to appear to us like one and the same ball, moving with an amazing velocity. In this way may, perhaps, be explained why balls of this description move horizontally, preserving nearly the same distance from the earth, and generally leaving a train of some miles behind them. That which appeared in 1783, was situated at a height of between 60 or 70 miles, and travelled a thousand miles, at the rate of twenty miles in a moment of time, frequently changing its colour from a bright blue to a dusky red. If, however, our barometrical calculations be correct, the air in these regions must be 4000 times rarer than on the surface of the earth; a tensity that will not suffice for combustion.

Electricity is not only influenced by caloric and light, but also by magnetism. This appears evident from the effects observed in those beautiful luminous corruscations termed the northern lights. They were not observed in our latitudes prior to the 15th century *, and were first mentioned by KEPLER. In the 16th century GASSENDI saw them, and gave them the name of *aurora borealis*, which they now bear. The great Dr. HALLEY, when an old man, mentions with pleasure, that, for the first time, he then saw those beautiful streams.

It has been supposed by some, that this phenomenon is hinted at in Maccabees †. In LUCRETIUS there is a description which bears some little resemblance to them; but the *Trabes* of SENECA is more particularly descriptive of this nature: " Trabes non transcurrunt nec prætervolant ut faces, sed commorantur et in eadem parti cœli collucent." If these were the same as our northern lights, the very accurate observers, Aristotle, Seneca, and Pliny, never mentioned their particular direction. They might be something similar to those lambent corruscations which are frequently seen in and about the Levant. Dr. Pococke tells us, that when he was in the Holy Land, he and his companions saw a gentle light approaching towards them, encircling them in its luminous embrace. In receding, it expanded, and enlightened many acres of the adjacent fields. Such little streams were frequently observed attached to the masts of the vessel in which they sailed. The ancients, who were superstitious in all celestial observations, when only one luminous stream appeared, called it *Helena*, as emblematical of an approaching storm. When two were united, or playing together, they deemed their voyage safe. These sociable twins they named *Castor* and *Pollux*.

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* They were first observed in 1574, November 14. No others, of any particular brilliancy, occurred till March 6, 1716, when several of them appeared for three successive nights, and were observed in England, Ireland, Russia and Poland; extending nearly 30 degrees of longitude, estimating the first meridian at London, and about 50 degrees of latitude. They were consequently visible all over Europe.

† B. ii. c. 5. " Through all the city, for the space of 40 days, there were seen horsemen running in the air in clothes of gold, and armed with lances, like a band of soldiers, and troops of horsemen in array, encountering and running one against another, with shaking of shields, and multitude of pikes, and drawing of swords, and casting of darts, and golden ornaments and harness."

With respect to the aurora, it happens rarely that its corruscations extend to 50 degrees of latitude. Why it should not have been seen before, is in some measure explicable by the observation, that the vertex of these corruscations always corresponds to the line in which no variation is marked by the needle. This line varies* in its direction, and is supposed by Dr. HALLEY to perform a revolution in about 6 or 700 years.

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* Sebastian Cabot, in 1500, discovered the variation of the magnetic needle. He merely supposed it to differ in different

In 1750, Mr. WARGENTIN observed that the needle was disturbed when the corruscations were very brilliant. WILCKE, VAN SWINDEN, and DALTON, have proved in what manner magnetism may be disturbed by electricity*.

About five or six years since the northern lights were very visible to us. As the line, in which there is not any variation, is at this period

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ferent latitudes; but our countryman, Mr. GELLEBRAND, first ascertained that it even varied at the same place. In 1580, Mr. BURROWES observed it at Limehouse 11° 15′ Eastward.—Mr. GUNTER, in the same place, in 1622, observed it 6° 13′ E. In 1661, Dr. CROME found it 45° 30″ W. of the North Pole; and since that period it has been gradually increasing, and is now about 23°½ W.—On account of this variation, to ascertain which is of the greatest importance to mariners, an azimuth compass is employed.

* The magnetic declination seems to undergo a diurnal variation with the state of electricity, as compared with the barometrical change, the afternoon declination being generally different from the forenoon declination of the same day. From many ingenious experiments made by the celebrated Mr. CANTON in 1759, it was found that the declination was, in the course of the observations of an entire year, increased about 10 or 12 minutes of a degree. This increase, he observed, happened principally in the summer months, the needle, during winter, being, in a great measure, stationary.

Vide Dr. LORIMER on Magnetism.

over the Atlantic Ocean, and as the northern lights can only be seen after sun-set, that particular part is below the horizon; and therefore will not be visible to us for at least 300 years*.

Whether electricity, magnetism, or caloric, ever exists in a pure, uncombined state, cannot be ascertained. Their various degrees of union will however be influenced by constant, operating, mechanical laws. If, after the celebrated ÆPINUS, we suppose that caloric, electricity, and magnetism, are three distinct principles differing in their gravities; that caloric is the heaviest, electricity the next heavy, and magnetism the lightest: if we consider them in this point of view, the following arrangement will take place, from the laws of centrifugal motion. While the earth revolves round its axis, all matter attached to it, whether directly or indirectly, will, by the aerial communication, participate in the same motion. Matter will, when in motion, have a tendency to recede from the centre in proportion to its momentum. Now, as the momentum is the quantity of matter multiplied by the velocity, the substance which is the heaviest will occupy the most remote part. As it is evident that the equatorial parts of the earth are the most remote from the axis, in this case the axis of the earth is to be considered as the centre of her diurnal motion; and the nearer we approach the poles, the less is our distance from the axis*. Upon these principles caloric being the heaviest, will occupy the tropical regions; electricity the temperate zones; and magnetism the polar or frigid zones.

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* Through a telescope tube fitted up with proper diaphragms, these lights may be, however, seen in the day-time. By thus preventing all lateral light, we are then enabled to distinguish them as clearly as any particular star.

Such a division appears in reality to exist. When, therefore, in the northern regions, any change takes place in the capacities of any aerial mass, as to electricity, this principle being disengaged, will, from the laws of motion, as being heavier than magnetism, dispossess the latter of its situation, and pass on, radiating, until it arrives at a part where it is balanced by resistance. In passing in this magnetic direction, the vertex must correspond with the line of variation of the magnetic needle.

Wilcke has observed, that needles acquire polarity by electricity. He found the South Pole to correspond with the part of the needle which refers to the positive communication; and the North Pole with that which refers to the negative. Notwithstanding M. D'ALIBARD contradicts this, the very ingenious BECCARIA observes, that if we place a needle in the magnetic meridian, the extremity towards the North will become the North Pole, and the one which is directed towards the South, the South Pole, upon sending a powerful charge of electricity through it. When the needles were perpendicularly placed, he always found that the inferior extremity became the North Pole. VAN MARKUM, assisted by the celebrated VAN SWINDEN, tried various experiments on watch-springs of 3, 6, and 9 inches long, having $\frac{1}{4}$ and $\frac{1}{2}$ inch in breadth, and about a line in thickness. When they were placed in the magnetic meridian, it was indifferent which end was connected with the positive side of the battery, the one which pointed towards the North, invariably becoming the North Pole. When they were sent through a magnetised needle, placed reversed as to its poles, not only the magnetic power of the needle became greatly diminished, but frequently the poles entirely changed. When a needle was placed perpendicularly, it was invariably found that the inferior part always became the North Pole, agreeably to the experiment of BECCARIA.

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* Supposing the circumference of the earth to be 24,000 miles, as a revolution takes place in 24 hours, an inhabitant on the equator will move after the rate of 1000 miles per hour. In the latitude of England the motion will not be more than 600 miles in the same time; and at the poles it will be nothing.

A weak magnet, thus situated, had its poles reversed. When the needles were placed in the magnetic meridian, they did not acquire more force than when they were perpendicularly elevated; and a second discharge often destroyed the effect of the first. When the discharge was so powerful as to heat the metal, it was rendered magnetical in a very slight degree; and when it was placed in the magnetic equator, it had no power at all. BECCARIA, who also tried this experiment, observes the whole of the side, throughout its length, became the North Pole. When the accumulated electricity was discharged through its breadth, the extremities acquired a greater power than in any of the preceding cases; the western extremity becoming the North Pole, and the eastern the South Pole.

On the influence of this universal principle of electricity, water-spouts seem to depend. Dr. STUART, who has given some excellent representations of water-spouts, supposes the water to have been drawn up by suction. Dr. FRANKLIN conjectures that a vacuum is induced by concentrated currents of air, and that the water is thus pressed up. SENNEBIER remarks, that they occur so rarely as to preclude an accurate investigation of them. MICHAUD has given an account of some curious water-spouts he observed at Nice, in December 1788. On the day to which he refers, a very violent storm of wind arose, attended by extreme cold. Early in the morning he observed a round space of about ten or twelve fathoms in diameter, at the distance of a musket shot from the shore, in which the water seemed almost disposed to boil. It moved before the wind towards the shore, and then overset, under the appearance of a long train of mist, and was speedily dissipated. At noon another fine water-spout appeared, so large as to suffice to cover a man of war of the first rate, with all her sails outspread. The lower part assumed the appearance of the crater of a volcano, and the water was elevated in parabolic streams from the centre to the circumference. While this was noticed, an impetuous shower of hail took place; upon more minute examination they appeared to be large flakes of snow, rounded by the wind in their fall. The water-spout, in moving towards the shore, gradually contracted in its dimensions, the base being reduced to nothing when it touched the shore, and the upper part having become broader and more rare. At length the whole joined the mass of clouds, in the same manner as one mist incorporates with another.

Water-spouts are, in general, about the size of the mast of a large ship. DAMPIER relates his having seen one so large, that, on its progressive motion, it passed over a ship becalmed on the coast of Guinea, and, having laid her on her broadside, carried away her fore-mast. An instant after, it passed to the other side and carried away her mizen-mast. It has been frequently observed, that in the dispersion of these water-spouts, light appears.

From these circumstances it would seem that these columns are formed by electricity. I have already observed, that the capacity of air for electricity becomes increased by the addition of caloric; and it may probably happen that, under some circumstances, changes of temperature may take place in such a way, as that electrical columns may be formed. If one of these columns should pass over the water, the same effects would take place as if an exhausted tube were stationed there. Water, being a conductor, is not resisted in its ascent; and hence the outer air will force it to the height of thirty feet, or until its column becomes a counterpoise to its pressure. The water it contains has never been observed to rise higher, when the spout arrives at the shore. In proportion as the base of the spout contracts, the resistance in the point of contact becomes the least; and when the electricity is removed, the air, rushing in first at the base, and then upwards within the column, occasions the dilatation *.

If these columns fall on a surface of the earth covered with non-conducting substances, such as the scorched sands of Arabia, the sands are then elevated, moving along with the wind, and constituting what are called the moving pillars of the desart. In those burning climes, the air is so dry, and at the same time insulated from the earth by the parched sand, that large extents of electric matter move almost in a pure uncombined state, appearing like a blush in the hea-

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* In large water-spouts of this description, the pressure must be very extensive, if produced in this manner. Some idea of this may be conceived, when it is considered that the aerial pressure is equivalent to 15 lb. upon every square inch. Supposing a water-spout of six fathoms, or thirty-six feet, in diameter, its pressure on a vessel would be more than 1000 tons.

In various parts of the globe are observed what are called fairy-rings. These consist of scorched annular marks on the grass, about four or five yards in diameter, and were formerly conjectured to arise from the glowing feet of the little fairies dancing in a ring. Mr. Nicholson, in his valuable Journal, has described several he observed in Kensington Gardens. These rings appear to be formed by a discharge of electric matter; and since combustion cannot take place without pure air, at the same time that electricity always displaces air, the parched surface must be confined to the boundaries of the electrical column, or, in other words, to the points where it unites itself with the air.

vens, and producing all the effects of an abstraction of air, by suffocating every animal exposed to their influence. The camel and dromedary bury, instinctively, the nose in the sand, and the inhabitants fall flat on the face, to avoid being immersed in the electric fluid. In this state it occasions such decompositions and combinations *, that its effects are felt across the Mediterranean as far as the shores of Italy, forming the Siroco of VOLNEY, or the Simoom of BRUCE.

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* It has been already observed, that the component parts of atmospheric air can, by the influence of electricity, be converted into an active acid. In our chemical inquiries, we can demonstrate the power of electricity in decomposing ammonia into its two component gases, viz. hydrogen and azote.

CHAP. XXIII.

The principle of galvanism explained; with a review of the different theories which have suggested themselves to physiologists—Discovery of the decomposition of water by the galvanic process—Chemical discoveries to which it may give rise.

WHEN this surprizing principle was communicated to the world, in the mode which has been already described, physiologists conjectured that it might lead to very important inquiries relative to the nature of nervous energy. The idea which had previously been suggested by WATSON, FREAKE, SHEBBEARE, and others, that electricity is an important agent in the animal economy, had received some additional support from the experiments of Mr. WALSH on electrical animals, and from the physiological remarks of Mr. HUNTER. Plausible as such theories appeared, many difficulties presented themselves to the electrician. At that period no idea could be formed of an accumulation of electricity in any substance, without such a substance being insulated. As the nerves are enveloped by good conducting media, the supposition of any partial accumulation in them, appeared contradictory to the phenomena of electricity.

By the earliest discovery of GALVANI, relative to the influence of an electrical spark* on the muscular fibres of a frog, it naturally appeared to him that this influence was produced by the disturbance of the electricity in the surrounding air. His subsequent discovery of the remarkable influence of two dissimilar metals, led him to the theory which he afterwards adopted.

Having noticed a peculiar sensibility in the muscular fibres of the frog, which he found susceptible of being acted upon by the minutest portion of electricity, he was stimulated to further inquiries. Having one day suspended a frog on the iron palisadoes contiguous to his house, for the conveniency of dissection, and a silver probe which he employed, now and then casually touching the palisadoes, he was surprized to find that, at every contact, the muscles were thrown into action. As he soon perceived that the same effects were not produced when his scissars or forceps formed the medium between the frog and the iron railing, he was led to suspect that the convulsions were produced by the influence of two different metals.

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* The frog was placed at some feet distance from the machine; and the spark having been received by one of GALVANI's pupils, its passage disturbed the electricity in the surrounding air.

Had GALVANI observed this influence of dissimilar metals prior to his discovery of the great sensibility of the muscular fibres to the minutest portions of electricity, he would, probably, have attributed the cause to the agency of some other principle.

These remarkable phenomena having excited the attention of philosophers, experiments were diversified, and different conclusions drawn. GALVANI, VALLI, ALDINI, VASSALLI, and CORRADORI, deemed the galvanic principle to be identical with the electrical principle; while FOWLER, CAVALLO and CREVE supposed it to be a principle altogether distinct.

It is unnecessary in this place to recapitulate the sentiments of these physiologists, their respective theories having been already particularized. Whatever doubts, in the infancy of this science, might exist as to the identity of galvanism and electricity, since the discovery of its accumulation by VOLTA, all difference of opinion seems to have subsided, its similarity to electricity being now generally admitted.

Notwithstanding the consonance of opinions as to the principle, the mode in which it is evolved has been differently accounted for. VOLTA supposes it to be produced by the destruction of the electrical equilibrium, and conjectures that the interposed fluid answers no other purpose than that of a conducting medium; while, on the contrary, FABRONI, Dr. ASHE, and others, apprehend certain chemical changes to be effected by the fluid employed, and suppose the electricity given out to be merely a chemical evolution.

In ascertaining the influence of a simple galvanic combination on animal substances, no one has been more industriously engaged than the celebrated HUMBOLDT, a philosopher whose ardour for knowledge, and whose capacious mind, have embraced almost the whole circle of the sciences, and who, on whatever ground he has trodden, has enriched it with his fruitful remarks. Notwithstanding he has occasionally wandered into the wild field of conjecture, still his conceptions operate upon the mind of the attentive reader, and, like the collisions of flint and steel, strike out a new train of pleasing ideas.

In the science of galvanism, VOLTA deservedly claims the pre-eminence. In the earlier period of its discovery, his penetrating mind nicely ascertained the electrical balance in different metallic bodies; and the disturbance occasioned by their contact, was evinced by his delicate electroscope. Unfortunately science has not yet been enriched with a distinct work of this eminent philosopher on animal electricity. His discoveries and sentiments have merely been communicated to the world through the medium of the philosophical journals. His communication to Sir JOSEPH BANKS, the distinguished patron of the sciences, gave to galvanism an importance it did not before possess. Although the effects produced by the contact of two dissimilar metals had excited considerable notice, still, from their being so limited in their application, the public interest was diminished to such a degree, that, at the time of VOLTA's discovery of the accumulation of the electric fluid, by galvanic processes, galvanism had almost sunk into oblivion.

This discovery forms a distinguished and interesting epoch in the science of galvanism. In the same way as VOLTA had explained the effects of two dissimilar metals upon a principle which he termed *moto-electrics*, so also the series of galvanic combinations he denominated a *moto-electrical* apparatus.

Philosophers soon perceived the extent of the application of this principle, to the explanation of many physical phenomena. Although CREVE had first shewn that water is decomposed by the simple galvanic operation, this was still more completely effected by the ingenious experiments of Messrs. NICHOLSON and CARLISLE. Mr.

DAVY *, although he has not enriched the science of galvanism by any new discovery, has, notwithstanding, been of essential service in reducing it to some kind of system. The powers of galvanism, as a chemical agent, are found to be very extensive. As all chemical decompositions depend upon the exility of the agent employed, and as, in general chemical processes, this agent is caloric; by the employment of a principle still more active and subtile, our analyses are likely to be more correct. Galvanism indeed holds out very flattering prospects. By its influence, decompositions are effected which were in vain attempted by caloric. HENRY, of Manchester, has, by its means, divided the con-

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* The circumstance of a galvanic combination formed of one metal and two dissimilar fluids, which has been deemed the discovery of this ingenious professor, was first communicated by VOLTA in 1794, in a letter to VASSALLI, and which was published by GREN in his Natural Philosophy. It is, however, very probable, that Mr. DAVY might not have known this, and that he is therefore equally entitled to the merit of the discovery. Thus, the chemical apparatus of WOULFE is now well known to have been described by GLAUBER. The experiments of PICTET on heat and cold, were published more than a century ago, by the Florentine academicians; the culinary discoveries of Count RUMFORD are detailed in an old German work; and in many parts of Germany similar economical stoves had been erected near a century ago, and are called *ſparrofens.*

stituent parts of ammonia, and exhibited the hydrogen and azote in their separate states. CRUICKSHANKS has produced the arbor Dianæ; and BRUGNATELLI his metallic electrats. GUYTON, in his observations on the sulphuret of antimony, supposes galvanism to have a considerable influence in the mineral kingdom. He thinks that the slow and progressive results of affinities may be put into action by the galvanic fluid. The celebrated crystalographist HAÜY discovered the influence of subterranean electricity; and the experiments of LICHTENBERG have demonstrated its energies, by the beautiful configurations produced on his electrophorus. It is probable that hereafter galvanism may be considered as a grand mineralizing agent.

It would seem that most of the silent operations of Nature are occasioned by the disengagement of this subtile principle. In inanimate bodies, it enables us to ascertain whether they are compound or simple substances. By its means we can determine whether two metals are dissimilar or not; whether metallic sulfures contain the metal in its reguline or oxydated state; or whether in any given substance carbon * is a constituent part.

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* It is worthy of remark, that carbon and metals resemble each other as to their varying capacities for electricity. Both of

From a variety of circumstances it appears that this principle is, above all others, a stimulus to the functions of the animal economy. The construction of the animal organization tends to prove that its arrangement is calculated for the employment of such a principle. To an important purpose of this nature seems to be devoted the function of respiration. As fluids are imperfect conductors, carbon is diffused through the venous system, and only undergoes its changes in the lungs, where the principle of electricity is imparted to the blood, by which it is enabled to stimulate the left ventricle of the heart *.

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of them, in their natural state, are good conductors of electricity, but, when combined with oxygen, each of them loses its conducting quality. All the oxyds of metals are non-conductors; and charcoal, when combined with oxygen, in the form of carbonic acid, or when constituting a true oxyd of carbon, as in the diamond, is no longer a conductor. It is doubted by some, whether the diamond is an oxyd of carbon. TENNANT thinks it is chrystallized carbon, and VAUQUELIN pure carbon.

* About four years ago I read a paper at the Physical Society at GUY's Hospital, in which I attempted to prove that respiration is not the cause of animal heat; but that the operation of the lungs appears to resemble a torpedinal action, and is probably arranged for the elicitation of some electrical principle. Since that time many new experiments have occurred, which have added additional force to this supposition, and which will be particularized in the sequel of the present work.

As the animal body has its non-conducting cuticle, so the vegetable kingdom possesses its equally resisting epidermis, with which the leaves and stalks of plants are guarded. The vegetable seeds are surrounded by a non-conducting velvet envelope, while the internal cellular and parenchymatous substance admits it ready diffusion.

It has already been shewn, that no changes can take place in conducting bodies, without a similar variation in their capacities for electricity. Thus, in the animal frame, any part being under a state of morbid derangement, its capacity for electricity will vary also; and thus, by a judicious application of this stimulus of nature, considerable good effects may be expected. The most indubitable instances have already been given of the blind and deaf recovering their sensory powers; of the paralytic patient being restored to his proper sensibility, and of rheumatic affections being almost instantaneously removed. Its direct influence on the principle of life has been fully proved, by its restoring deranged persons to the enjoyment of sense and reason, and counteracting the dreadful effects of hydrophobic irritation. Galvanism appears to be an energising principle, which forms the line of distinction between matter and spirit, constituting, in the great chain of the creation, the intervening link between corporeal substance and the essence of vitality. By this cursory view we may perceive the very extensive range of the influence of this principle; and if, at so early a period, so much has been ascertained, what may not be expected from the united efforts of those who are interested in the progress of the sciences?

CHAP. XXIV.

On simple galvanic combinations—The arrangement of metals which forms the most perfect combination—Galvanism and electricity identically the same principle—The different modes in which they are produced—How far the development of electricity is a chemical process—The influence of dissimilar metals on the muscles of a frog—Any two different metals may form a galvanic combination—Table of conductors and non-conductors.

TWO dissimilar metals, and an interposed fluid; or a single metal, exposed to the action of two different fluids; or any one of the conducting substances on which unequal actions can be induced by different fluids, constitute a simple galvanic combination.

A series of such combinations is denominated a galvanic battery.

When two metals are employed, they produce the greatest effect, when the particular quality of one of them renders it extremely difficult of oxydation, while the other unites with oxygen with the greatest facility.

If, upon a polished plate of steel, water be laid, under the common temperature of the atmosphere, in the course of two or three days the surface will be considerably oxydated. This is occasioned by the decomposition of the water.

If, upon a similarly moistened surface of steel a plate of zinc be placed, at the end of two or three days the surface of the steel will be found covered with a white powder, which has been ascertained to be the oxyd of zinc. When it is wiped off, the steel will still possess its original polish.

This proves that the zinc unites more readily with oxygen than does the iron; but if a plate of silver be substituted to the zinc, then the steel will be oxydated*.

Consequently, silver is more difficult of oxydation than steel, and steel than zinc.

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* If a metal be placed in a vessel of dry oxygen gas, no oxydation is formed. Oxygen is more readily separated from water than detached from its gaseous state. When a series of galvanic combinations are placed under a receiver, the oxygen is rapidly separated; but if the receiver be exhausted of air, the process is very slow. It would appear from hence, that oxygen, in its gaseous state, cannot enter into an union with a metal, until the latter be thrown into a state adapted to its admission, either by the agency of moisture or of caloric, &c. The pure air is in this manner so completely separated, that Guyton was induced to employ the Voltaic pile as an eudiometer.

Silver and zinc, or gold and zinc, form the most perfect galvanic combination.

All metallic bodies are excellent conductors of electricity; but, when oxydated, they become non-conductors. In this change of capacity electricity is given out.

Although galvanism and electricity may be considered as the same principles, still, according to the present state of our knowledge, they may be thus distinguished.

Galvanism is the portion of electricity which forms a component part of the conducting body, in the act of undergoing a change in its capacity, from a greater to a less state; while electricity is the result of a temporary change in non-conducting bodies, insomuch that their capacities become, by attrition, momentarily increased.

Galvanism is never produced by any changes in non-conductors; while electricity is accumulated by them alone.

A metallic body, when insulated, undergoes very slow changes, compared with those produced on it when non-insulated.

A piece of zinc, placed in a weak solution of sulphuric acid, is subjected to but a trifling alteration, compared to the change which ensues when it is brought in contact with a piece of silver. This curious phenomenon was first noticed by Dr. ASHE; and was by him advanced as an argument in favour of galvanism being the result of a chemical operation.

A plate of silver, and a plate of zinc, with a piece of cloth, or of any bibulous substance, moistened in water, placed between them, constitute a simple galvanic combination. In this case the zinc becomes oxydated on the portion of its surface which is in contact with the wet medium.

If a prepared frog* be substituted for the moistened cloth, the humidity on its surface will produce similar effects on the zinc; and when the circuit is completed between the silver and zinc, by any substance which is a conductor of electricity, the muscles of the frog will be thrown into action. These contractions will be renewed every time the circuit is completed.

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* By a prepared frog is understood, a frog divided into two portions by a pair of scissars, through the middle of its body and spine, the viscera being removed, and the lower part skinned. The sciatic nerves rise very high up in the spine in this animal, and are very discernible when it is thus arranged. When it is intended to arm the nerves, after the introduction of a pair of pointed scissars beneath them, the spine is cut through without dividing them. A portion of the inferior part of the spine should be afterwards separated; so as to leave room for the nerves to be covered by a piece of tin-foil. This is what is denominated *arming*, or *coating* the nerves. In some experiments, it is more convenient to separate the lower extremities, and employ the crural nerve.

If a single metal be employed, it must be of such a description as not to be easily acted upon by water. Thus, if a silver cup, nearly filled with a weak solution of nitric acid, be placed in a glass vessel containing common water, a simple galvanic combination will be formed; seeing that the inside of the silver vessel will become oxydated by the acid, while its external surface will not undergo any change. If a prepared frog be made to form the circuit between the two fluids, immediately on its completion the convulsions will be produced.

Dr. WELLS says, that two homogeneous metals will produce contractions, if one of them be rubbed by silk, wood, or sealing-wax. This effect I have never observed, although I have tried every mode of excitation. It probably arose from the metals containing an alloy.

FOWLER and PFAFF assert, that the conducting powers of metals are in no way influenced by temperature. HUMBOLDT says, on the other hand, that a moderate increase of warmth does influence them. I have tried zinc, from the temperature of 40 degrees of FAHRENHEIT, to near 300, without observing any difference.

The more active the fluid which is employed, the more powerful will be the effects. Thus, salt water is better than common water; and acid and water than salt and water. VOLTA ascribes this difference to the conducting powers of the fluids, and not to any changes induced in the metallic bodies employed.

After a certain period, the metal the most liable to oxydation has its surface oxydated; and till this is removed the galvanic operation ceases.

Whenever a metal becomes oxydated, its capacity for electricity is diminished: oxygen cannot enter into union with a metallic substance, without the electricity which constituted a portion of the metal being given out.

A metal which is in a state of giving out electricity, will evince positive signs of the electric fluid, and occasion a sensible electroscope to diverge with the same state of electricity as is produced by excited glass.

In the series of metals, of gold, silver, copper, iron, tin, lead, zinc, each will become positive by its combination with that which precedes it, and negative with that by which it is followed.

Thus, gold and silver form a simple galvanic combination. If a fluid be employed which will act on the silver, and not on the gold, as happens with the nitric acid; in this case the silver will be the oxydable metal, and will therefore give out a portion of its combined electricity.

This equally applies to copper and iron, to iron and tin, &c.

Silver, when combined with gold, exhibits positive signs; but when it is arranged with copper, it evinces negative signs.

As all metals, with the exception of gold, undergo some small changes on the application of an oxydating fluid, the most perfect galvanic combination is constituted, when one of the metals undergoes the greatest change, and the other the least.

When zinc and copper are employed with a solution of a muriate of ammonia, the muriatic acid acts upon the zinc, and a portion of ammonia attacks the copper. In this way the galvanic effect is not so powerful as if gold had been employed.

All substances which are conductors of electricity are likewise conductors of galvanism, and in precisely the same order: the most perfect conductors being metallic substances; next charcoal, muscular flesh, &c. nearly in the following order:

CONDUCTORS.

All the metals.
Minerals containing metals not oxydated.
Vegetable coal.
Mineral coal.
Carbonated blend.
Aluminous schist.
Inflammable schist.
Siliceous substances containing carbon.
Grey manganese, arising from the carbon it contains.
Muscular flesh.
Membranes, nerves, ligaments and blood-vessels, whether moist or dry.
Morells and mushrooms.
White of an egg.
Water, blood, juices of plants.
Cellular tissue of vegetables.
Spirit of wine.
Wine, beer, acids, alkaline solutions, soft soap, and flame, (doubted by some).

NON-CONDUCTORS.

Oxyds of metals.
Oxyds of carbon.
Gases.
Periosteum.
Hair.
Epidermis and cuticle.
Fibres of wood.
Glass.
Amber.
White of an egg hardened.
Oil.
Resins.
Gums.

CHAP. XXV.

The influence of two dissimilar metals on the muscular fibres of a frog—How the electricity of the metals is disturbed—The oxydation cannot go on till there is a conducting medium from one metal to the other—The exquisite sensibility of the fibres of a frog to electricity, compared to the gold-leaf electroscope.

VOLTA divides the conductors of electricity into two classes, namely, dry and moist. The dry conductors he ranks in the first class; and the moist conductors in the second. When a moist conductor, and a dry conductor, are brought into contact, a disturbance of the electrical fluid takes place. The mode in which this disturbance is produced, he does not venture to conjecture; but says in his Letter to Professor GREN, "Do not ask me in what manner, it suffices that such an impulse is produced."

VOLTA supposes that all the fluid media act merely as conductors of electricity. That water, holding in solution an acid, produces more powerful effects than simple water, he attributes exclusively to the superior conducting power of acid substances.

When two dissimilar metals are employed, such as a plate of silver and a plate of zinc, after frequent use, the zinc becomes changed on its surface, loses its metallic splendour, and has an oxydated appearance. The experiments of ASHE and FABRONI also tend to prove certain chemical changes effected by the contact of two dissimilar metals with an interposed fluid.

In *Plate* V. *Fig*. 1, suppose Z to represent a plate of zinc, S, a plate of silver, and F, a prepared frog lying between them. Upon completing the circuit between the silver and zinc plates, by the means of the conductor C, the muscles of the frog are instantly thrown into violent convulsions; and these actions are repeated as often as the contact is made by the conducting wire and the two metals.

When the prepared frog is in a desiccated state, without any humidity on the portions in contact with the two plates, no contractions ensue, on the circuit being completed.

The zinc plate becomes oxydated by the moisture on the surface of the frog. In this process a quantity of electricity is given out. Unless the surface be in contact with a conducting medium, a very small portion of electricity can be disengaged; and the oxydating process cannot continue, because the oxygen cannot enter into union with the metal, without

out electricity being given out. The whole is to be deemed a chemical process; but we cannot with propriety say that the disengagement of electricity is the effect or the cause of the other phenomenon. They are contemporaneous results.

When there is not any conducting medium to communicate the disengaged electricity, it acts as a resisting power to the further union of oxygen with the metallic substance, and consequently prevents the oxydation. In the above cited experiment, made by Dr. Ashe, if zinc be thrown into a diluted solution of sulphuric acid, the process is much slower than when a piece of silver is in contact with the zinc.

In consequence of this oxydifying process, the natural electricity of the zinc becomes disturbed; and the direction in which it will have a tendency to move, will be towards the particular surface which is undergoing the change in capacity.

If a plate of zinc be supposed to be divided into a number of lamina or strata, the superior stratum being thrown into a state of readily parting with its combined electricity, and the successive strata being similarly acted on, the eleccricity in the plate will be thrown into motion, and the direction will be, from the opposite side of the plate, towards the side which is undergoing a chemical change. In consequence of this disturbance, the side of the plate of zinc which is not exposed to moisture, or to the action of any fluid, will be in a negative state, or, in other words, will contain less than its natural quantity of the electric fluid, while the other side will be in a positive state of electricity.

Electricity, like every other description of matter, when thrown into motion, does not possess in itself any specific powers to recover its original state. It will always remain quiescent, unless influenced by external causes; and, when thrown into motion, will invariably move in the direction where there is the least resistance.

The power which originally causes the disturbance, producing at the first instant its most active effects, becomes more and more feeble at every successive instant, until at last its energy finally ceases. It would require a similarly active power, to again restore the electricity to its original state.

When a conducting medium is placed between both sides of a plate of zinc, then an equalization is effected. The surface under the chemical action of the fluid, being freed of its excess of electricity, and the other side recovering its original state, the oxydifying process again goes on, and thus continues, so long as the metallic arc completes the circuit between the sides.

As zinc undergoes this change with so great a facility, in order to produce a perfect galvanic combination, it is necessary to guard and defend one of its sides from any change, by coating it with a metallic substance, the capacity of which for electricity is not so easily altered, such as gold, silver, copper, &c. These metals, when soldered to zinc, are not otherwise useful, than because they are less acted on by the fluid, than the zinc would have been; and hence, as to the state of electricity, they may be simply deemed to accord with the zinc surface with which they are united.

If, instead of two metals in immediate contact, a piece of animal substance be interposed, as in *Fig.* 1, *Plate* V. then the silver will be in the negative state, and the zinc in the positive; and these states will be acquired in the gradual manner before particularized, namely, by the action on successive strata. When the equalization is effected by a metallic conductor, as C, a fresh portion of electricity instantly passes from the silver, through the conducting medium of the muscular fibres of the frog, to the zinc. In this transmission the fibres are thrown into a contractile state.

The muscular motions are not produced by the equalization of the zinc and silver, at the instant of the circuit being completed; but, in consequence of this completion, the oxydifying process at the same instant going on, the silver, which is at this precise juncture restored to its original state, parts with it as at the first instant. I have considered these processes as all taking place at one and the same instant, which is not mathematically correct. But as the changes occur with so great a rapidity as not to be followed by our senses, they consequently appear to us to take place at the very same moment of time.

The muscular fibres of a frog are so extremely sensible to the influence of electricity, as to form the most delicate electroscope hitherto known. The great sensibility of the gold-leaf electroscope, the elegant contrivance of Mr. BENNET, is well known; but, notwithstanding the great improvement of Mr. READ's condenser, it evinces no disturbance by the simple contact of two dissimilar metals. When a large plate of zinc, and a similar plate of copper, having insulating handles adapted to them, are brought into contact about twenty times, and one of the plates applied, on each contact, to the condenser, upon removing the latter, the leaves of gold are seen to diverge. The state of electricity with which they diverge, is invariably positive, if the zinc plate be applied to the electroscope; and negative if the application be on the side of the copper-plate.

In this case, the plates must be at least five or six inches in diameter. But we see, on the other hand, that a very minute portion of zinc and silver will produce violent convulsions in the limbs of a frog.

If ten persons* moisten their hands with salt and

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* GREN, in his valuable Elements of Experimental Philosophy, which, unfortunately for science, have not yet been translated from the German into the English language, brings forward, in section 1404, the following particulars communicated to him by the celebrated VOLTA :—" The electric fluid, when set free, and put in motion, is a stimulus to the animated fibre; and the effect of its irritation, while actually pervading that fibre, consists of a sensation or motion produced in it. The muscular fibres constitute the most sensible electroscope, and shew the presence of an electrical stream, which is not able to disturb the most delicate electrometer," &c.

From this quotation alone the sentiments of this celebrated philosopher are easily known. In the subsequent section GREN has remarked how the sensations of seeing and tasting may at the same time be produced, by a very striking experiment, which is as follows :—" The muscular motion, as well as the sensations of seeing and tasting, may be produced at the same instant, by the means of the electric stream. Let four persons stand upon a somewhat imperfect conductor, and communicate between themselves in this way. Let the first person hold in his right hand, well moistened, a cylinder of zinc, and touch with the finger of his left hand, the point of the finger of the second person, who, again, should touch with his finger the bare eye-ball of the third: now, let the latter hold the legs of a prepared frog in his well moistened hand, at the same instant that the fourth person, with his moistened right hand, lays hold of the rump of the frog, holding in his moistened and water, and form a circuit, by holding the hands of each other; and if, in this circuit, a prepared frog be placed, one of the persons holding the animal by the feet, and another by the spine, at the same time that the first person in the circuit has in his disengaged hand a half-crown, and the last person a plate of zinc, held in the same way, each time the two metals are brought in contact, the frog will become convulsed.

I shall demonstrate, in a subsequent part of this work, the very feeble power of a large galvanic battery, exposing a surface of four thousand inches, when the fluid passes through such a circuit of imperfect conductors as the human body. The hands being guarded by a non-conducting cuticle, each point of contact in the circuit acts as a resisting force.

If the accumulated electricity, from such an extensive surface, becomes so weakened as not to act with a twentieth part of its power, through a circuit of this kind; how surprizingly sensible to

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moistened left hand a cylindrical piece of silver. If the first and last of the persons who form this series, bring the zinc and silver into contact, and in this way complete the circuit, the person whose tongue is touched will perceive an acidulous taste; the touched eye of the third person will perceive a flash of light; and the thighs of the frog, which are held by the third and fourth persons, will be violently convulsed."

to electricity must be the muscular fibres of a frog, seeing that they are influenced by a twentieth part of the very minute portion of the fluid, arising from the contact of two small dissimilar portions of metal.

Some faint idea of this may be formed from the following calculation: I employed two square pieces of silver and of zinc, each measuring on either side one-tenth of an inch, and having consequently an area of the $\frac{1}{100}$ of an inch only: on bringing them into contact with a prepared frog, convulsions were produced; the connecting wire employed to complete the circuit being only the $\frac{1}{200}$ part of an inch in diameter.

Two circular plates of copper and zinc, six inches in diameter, required to be brought twenty times in contact with the condenser, before any sensible disturbance of the gold leaves could be induced. The area of each plate being $6 \times 6 \times .7854 = 28.2744$; this product, multiplied by 20, the number of times the contact was made, will be found to be equal to 565.488. The sensibility of the fibres of the frog to that of the condenser was, consequently, as 565.488 is to $\frac{1}{100}$, viz. 56548.8, or more than 56,000 times greater, in point of sensibility, than the delicate instrument made with the gold leaves.

My ingenious friend ALDINI, well known to the public by his galvanic experiments, considers the principle to be inherent in the animal fibre alone, and endeavours to oppose the Voltaic doctrine, which makes it arise from the heterogeneity of the metals employed. He adduces two experiments to elucidate his opinion: Supposing several prepared frogs, ten or more for instance, to be arranged in a parallel line to each other, as in *Plate* V. *Fig*. 5, in such a way as that the nerve should be at one end, and the feet at the other; upon the circuit being completed between the coatings applied to one of the frogs only, convulsions are excited in all the others.

If this experiment be repeated, as in *Fig*. 6, when the frogs are reversed, as to each other, and disposed alternately, the convulsions do not ensue in all of them.

I have repeated these experiments with the utmost care, without their being attended with the results ALDINI has described. When the frogs were placed on glass, by studiously avoiding any conducting medium between each of them, such as moisture, &c. I invariably found that the one only which was in contact with the coatings was convulsed.

If there be any humidity on the surface of the table on which the animals are placed, a regular communication will be formed between each of the frogs and the coatings: consequently, upon the equalization, they will be all alike disturbed.

ALDINI and HUMBOLDT have both observed that, if the legs of a prepared frog be immersed in mercury, and the nerves be brought in contact with the external surface of that fluid, the convulsion will take place every time the circuit is formed.

From this experiment these physiologists have deduced, that dissimilar metals are no ways necessary. The celebrated VOLTA properly observes, that although one metal is thus apparently employed, the different states in which the mercury is found, constitute what may be considered as two dissimilar metals. As mercury, the instant it is exposed to the air, becomes oxydated, a part of the animal being in contact with the fluid in its metallic state, and the sciatic nerve being equally so with the oxydated surface, convulsions will necessarily ensue.

From all the experiments hitherto brought forward, in which convulsions have been produced by the influence of dissimilar metals, it would appear that the disengaged electricity is from the oxydated surface of the metal; and that the muscular fibres of the frog are, in consequence of their exquisite sensibility, convulsed by the transmission of the electricity from the non-oxydated plate to the oxydated plate.

The disturbance of the straws, produced, according to VALLI, by a battery of fourteen frogs; and the disturbance of the gold leaves, which KÜHNE fancied he perceived, appear to have originated from some other cause *. This is, at the least, conjectured by VOLTA, FOWLER, CREVE, LICHTENBERG, SCHRADER, PFAFF, and HUMBOLDT. The hairs of a mouse were said by VALLI to have been erected by a similar process; but HUMBOLDT found this to be perfectly erroneous.

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* Owing to the very great sensibility of BENNET's electroscope, signs of disturbance are evinced upon the slightest agitation. The wafting of a few dusty particles produces, as well as the evaporation of a drop of water, a divergence of these leaves. It is therefore probable, that their separation, as it was observed by KÜHNE, might originate from some such slight agitation.

CHAP. XXVI.

On the influence of a single metal and two dissimilar fluids, on the muscular fibres of a frog—Observations of Professor ROBISON *on galvanic combinations—The discoloration of a silver spoon from the eating of eggs, explained—The combination of a single metal and two dissimilar fluids—The discovery of* VOLTA *announced in a letter to Professor* GREN.

I HAVE already observed that a simple galvanic combination may either consist of two dissimilar metals, and an interposed fluid, or of a single metal placed between two different fluids, one of which acts on the metal without its being influenced by the other.

Let A B, *Fig.* 1, *Plate* VI. represent two glasses, one filled with a diluted solution of nitrous acid, and the other with water. If these two vessels be connected by a copper arc, *a b*, a simple galvanic combination will be formed. The part of the copper in the diluted solution of nitrous acid will be oxydated, while the portion in the vessel of water will not be acted on. If one of the extremities of a prepared frog, as at F, be

F, be placed in one of the liquids, as soon as the other extremity is brought in contact with the liquid in the other vessel, convulsions will ensue.

This effect is more strongly produced in the experiment represented by *Fig.* 2, *Plate* VI. in which B D is a glass dish filled with a solution of the sulphuret of potash, and A, a silver cup, or a salt-cellar, placed withinside, and nearly filled with a weak solution of nitric acid. By this arrangement a perfect galvanic combination is produced; and as soon as a prepared frog is made to form a communication between one of the fluids and the other, it will be convulsed.

The principles of this combination are precisely the same with those of the one precedently noticed, in which it must have been observed, that one side of the metal is acted on by the fluid, while the other side is guarded by a coating of another metal, on which the fluid does not produce any action. In the latter case, the active liquid is simply in contact with one of the surfaces, or parts, of the metal, while the other surface, or part, undergoes no disturbance.

Upon the principles of simple galvanic combinations, many curious phenomena are explicable. Formerly, in applying copper sheets to the bottoms of ships, it being the usage to employ iron bolts, it was observed that the copper soon became loosened in the part where the two metals were in contact. This arrangement constituted a simple galvanic combination of two dissimilar metals, and an interposed fluid. In this case the interposed fluid was the sea water, or a solution of the muriate of soda, and the metallic substances, the iron and copper. In a similar arrangement, the iron, the most oxydable metal, in becoming oxydated in the part more immediately in contact with the copper, is soon destroyed, and detached from the latter substance.

In 1795, CREVE first shewed that, in every simple galvanic combination, water is decomposed. The oxygen, in these combinations, enters into union with the metal; and the hydrogen is evolved.

Professor ROBISON has remarked, that the drinking of porter out of a pewter pot, produces a more brisk sensation than when it is taken out of a glass vessel; and this he ascribes to a galvanic effect. In this instance, there is a combination of one metal and of two dissimilar fluids. In the act of drinking, one side of the pewter pot is exposed to the saliva and the humidity of the mouth; while the other metallic side is in contact with the porter. In completing the circuit, in the act of drinking, a brisk and lively sensation arises, which imparts an agreeable relish to the porter.

This ingenious professor has carried the refinement of taste so far, as to fancy that snuff, taken out of a metallic snuff-box, excites a more agreeable sensation, than when taken out of a box of tortoise-shell, or *papier maché*.

The discoloration of a silver spoon, in the act of eating eggs, is likewise a curious galvanic operation.

Mr. Carlisle, by a very ingenious analysis, has proved the existence of sulphur, both in the yolk and in the albumen of an egg. A tea-spoon left immersed in either of these substances, undergoes no discoloration; but the above remarkable phenomenon takes place in the act of eating the egg; the extremity of the spoon which is most in the mouth, being always found to be the most discoloured.

In this case the galvanic combination consists of the sulphurated solution, the silver tea-spoon, and the saliva. In every galvanic operation Creve has shewn that water is decomposed. The nascent hydrogen, readily uniting with the sulphur, forms sulphurated hydrogen gas, which produces the discoloration. It is this gas, evolved from our coals in the act of combustion, which occasions a similar tarnish to the silver employed for domestic purposes.

Volta, to whom we are indebted for the discovery of the galvanic combination of a single

metal

metal and two different fluids, observes, that if a tin basin be filled with soap suds, with lime-water, or with a strong ley, and if it be held by both the hands, moistened in pure water, upon applying the tip of the tongue to the fluid, an acid taste will be perceived. In proportion as this acid sensation gradually dies away, the taste becomes pungent and alkaline.

These curious changes of sensation invariably occur; but VOLTA has not undertaken to reason upon the cause. It may perhaps be deemed presumptuous in me to hazard an opinion, when so profound a physiologist has hesitated to pronounce his. I submit, however, as mere conjectures, my sentiments on this head.

From what has been before observed, it appears that when water is decomposed, the oxygen is evolved. This principle, by uniting with azote, may form nitrous acid; and the evolution of oxygen may thus produce an ascescent state, while the nascent hydrogen, uniting with azote, may, by forming an ammonia, account for the alkalescent sensation. In a subsequent chapter I shall take an opportunity to observe, that, according to every probability, ammonia and nitrous acid are formed in the greater part of the galvanic combinations.

VOLTA, who has not as yet favoured the world with his collected opinions, they being scattered and diffused through different journals and periodical works, in such a way as to render it difficult to arrange them in a systematic order, observed, in the year 1794, in a letter to Professor Gren *, that the contractions also take place instantly, if one end of the connecting arc be moistened with a conducting fluid of a different kind; such, for instance, as water is with regard to an alkaline solution, to dilute nitric acid, or to a solution of an alkaline sulphuret; or if into one glass water be poured, and into the other vinegar, or a solution of an alkali or of a sulphuret, or of a salt; and if in this experiment the communication be made by means of one single metal only.

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* Allein in dem angefüthtem falle (§ 1393) sind die zuckungen gleich wieder da, wenn man das eine Ende des leitenden bogens mit einer leitende fluessigkeit anderer art, als blosses wasser ist, z. b, mit einer aufloesung von alkali, mit scheidwasser, mit einer aufloesung von schwefelalkali bestreicht; oder wenn man in das eine glas blosses wasser, in das andere essig, oder eine alkalische aufloesung, oder eine aufloesung von schwefelleber, oder eine salzanfloesung giesst, und die verbindung *jetzt anch nur durch ein einziges metall* macht.

CHAP. XXVII.

On the influence of charcoal—Mr. Davy's *galvanic combination with charcoal—Experiments made on that substance—In the order of the conductors, it stands next to metals—Why it varies in its properties—Opinions of* Pfaff, Humboldt *and* Laghi*—Remarks of* Fowler *and* Wells—Abilgoard's *proportion of carbon in venous blood—Observations on the charring of wood.*

MR. DAVY, the chemical professor at the Royal Institution, whose ingenious galvanic experiments are well known, observes, that charcoal possesses a similar property with metals, of forming a galvanic combination with two dissimilar fluids. Supposing, according to him, A and B, *Fig*. 3, *Plate* VI. to represent two glasses, one filled with nitric acid, and the other with water: if a communication be formed, by means of a curved piece of charcoal, C D, a galvanic combination will be produced, sufficient to disturb the muscular fibres of a frog.

He also remarks, that a solution of the sulphuret of potash, and the sulphuric acid, will, with the aid of charcoal, form a similar combination.

I have repeated these experiments with the utmost care, but have not been equally successful with the above physiologist. When a prepared frog is so applied as that the leg is in one of the glasses, and the crural nerve in the other, slight contractions will be produced. But if the muscular part only be in each of the glasses, there will not be, at least as far as I could observe, any sensible motion.

In this case, I conjecture the galvanic combination to consist of the two liquids, and the muscle and nerve; the charcoal merely acting as the conducting medium.

I am so fully aware of the extreme difficulty of procuring good charcoal, that I do not presume to deny the results of Mr. DAVY's experiments. The charcoal employed by him was made from box, or lignum vitæ, well charred. These woods, being dense, do not absorb water in so great a degree as the others.

Charcoal is an excellent conductor of electricity, and ranks next to metallic substances. It has been asserted, that it was first employed by FONTANA as a conductor of galvanism; but the earliest experiment was made by VOLTA. PFAFF observes, that when a piece of charcoal is found not to be a good conductor, it is rendered efficacious

cious by the process of combustion. HUMBOLDT remarks, that this method has never succeeded with him; and thinks that the impediment is occasioned by the carbon being enveloped in the hydrogen, the fibrous part of the wood containing that principle, as well as carbon, oxygen, carbonate of magnesia, calcareous earth, and a small portion of azote. The alkali which arises from the combustion of plants, is, notwithstanding the inference drawn from their experiments by MARGRAAF and WIEGLEB, formed posteriorly to the decomposition, by a fresh combination. Hydrogen, at similar temperatures, more readily unites with oxygen than does the carbon; and hence, water is first formed, and afterwards the carbonic acid.

Charcoal, when divested of its hydrogen, has a black radiated appearance, and is then best calculated for galvanic experiments. Professor LAGHI, of Bologna, attributes the non-conducting quality of some particular charcoals to resin or bitumen not properly liberated by combustion. In pieces of imperfectly conducting charcoal, he observed parts not well charred, which on that account cut off the galvanic circuit.

The conducting powers of minerals were first ascertained by Dr. FOWLER, a very ingenious physician of Salisbury, whose valuable observations have already been detailed. These substances

stances were found by him to be conductors, in proportion as they contained metallic or carbonaceous matter.

HUMBOLDT having noticed that the Lydian stone is a conductor, suspected it to possess that property in consequence of a certain portion of carbon it contains. On burning it with caustic potash, in an open vessel, the potash became mild, and effervesced with acids. A proof was thus obtained of its having acquired carbonic acid.

Dr. WELLS, whose sentiments always merit the highest respect, supposes that charcoal, recently made, is in its most perfect conducting state. The opinions of this very intelligent and well informed philosopher, on subjects generally relating to galvanism, have been recorded in a former part of this work.

From these observations, we may perceive how delicate a test galvanism is, in detecting carbon in a substance in which the presence of that principle was not even suspected. So great, at the same time, is the sensibility of galvanism to good conducting media of this description, that it evinces instantaneously the existence of the smallest portion of carbon.

All substances which have this principle diffused through them, become galvanic conductors.

Professor ABILGOARD has remarked, that one ounce of venous blood contains 115½ grains of charcoal; while the same weight of arterial blood contains 87½ grains only.

This remarkable circumstance I shall have occasion to notice more particularly in my observations on respiration.

Some persons have supposed galvanism to differ from electricity, because the conducting powers of charcoal have been found to be greater for the former than for the latter principle. This is, however, far from being the case. Charcoal preserves the same rank in the order of conductors, both for electricity and for galvanism. That it is found superior to metals, in the deflagration of the latter substances by galvanic processes, does not depend upon its conducting power, but upon its aiding principle of combustion.

To prepare the best charcoal, I have found the following method the most effectual: Having laid in a crucible pieces of boxwood or lignum vitæ, about one inch in thickness, when they are nearly heated to redness, the crucible should be covered, in order to prevent the access of the external air, since otherwise combustion would ensue. By continuing the heat till the crucible is red-hot, and then removing it from the fire, in order that it may be gradually cooled, the wood will be found to be properly charred.

The principal precaution in this process, is to make choice of the proper time when the crucible should be covered. The wood should previously be sufficiently heated, in order that every particle of moisture should be thrown off. Wood, when charred, loses three-fourths of its weight, and about one-fourth of its bulk. FONTANA and MOROZZO have remarked that charcoal, in cooling, absorbs both air and moisture. CHAPTAL observes, that desulphurated pit-coal, or coak, absorbs one-fourth of its own weight of water; and charcoal about one-fifth. Box and lignum vitæ produce a charcoal which is hard, sonorous and brittle; while the other woods in general yield a substance light, spongy, and friable, and on that account more disposed to the absorption of water, in the act of cooling. These circumstances point out the propriety of letting the charcoal cool before it is removed from the crucible, and likewise of keeping it afterwards in a covered vessel.

Carbon[*], when mechanically diffused through bodies, possesses its conducting properties; but when it is in chemical union with them, it undergoes a change in its capacity. Thus, in the case of the carbonates of lime, of barytes, of magnesia, and of alumine, its conducting faculties are lost; and this also happens when it is united with hydrogen, so as to form hydro-carbonate; when, being blended with oxygen, it constitutes carbonic acid; and when, in the intermediate state, it forms the gaseous oxyd of carbon.

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[*] Both alcohol and oil consist of oxygen, hydrogen, and carbon; notwithstanding which, the former is a conductor, but not the latter. This is not owing to the carbon, of which oil has a greater proportion than alcohol; but because the latter fluid, in its purest state, contains water, as has been demonstrated by LAVOISIER and MEUNIER.

CHAP. XXVIII.

On the influence of animal substances, in constituting galvanic combinations—The experiments of Aldini *explained on the foregoing principles—Whether galvanism is connected with the nervous system—Observations of naturalists on the nerves of insects—On fungi as conductors—How a ligature on the nerves influences galvanism—Remarks on the reproduction of nerves, when divided—Conjectures on the infinitude of our sensations.*

VOLTA, with the philosophical acumen he so singularly possesses, has observed that various parts of animals can be excited without the aid of metals; and several ingenious methods have been proposed by Galvani for this purpose. In the opusculi of Milan, is an account of Aldini's experiments, made in the presence of the Institute of Bologna, as early as the year 1794. In the memoir of this physiologist, addressed to Amorotti, he describes one of these experiments as follows: A prepared frog having been immersed in a strong solution of the muriate of soda, and removed from thence, the learned professor held one of the extremities in his hand, and allowed the other to hang freely down. While the animal was in this position, he raised up the nerves with a glass rod, on suddenly removing which, as often as the nerves touched the muscular parts, convulsions were produced.

This experiment having, with several other analogous ones, been repeated in the presence of the National Institute at Paris, it was there observed, that they would have been more correct, if the nervous and muscular systems had been perfectly insulated. To prove that these contractions did not arise from any defect of insulation, ALDINI made the following experiments:

Suppose L N, *Fig.* 7, *Plate* V. to represent a prepared frog; S the spine; R the sciatic and obturator nerves; and L and N the legs. The whole having been previously moistened in salt and water, and the frog being supported by a glass rod, H, at the spine, S, while another glass rod, M, raises up the leg, L, until the muscles are brought in contact with the nerves at R, immediately on the contact being made, the convulsions take place. In this experiment no part of the frog is otherwise touched, than by non-conducting media. This has been more correctly tried by the means of two insulated frogs, the feet of one of which were tied to the spine of the other.

Let MN, *Fig.* 4, *Plate* V. represent a glass rod; and A, B, two prepared frogs; the feet of B being secured to the spine, C, of the frog, A. On the sciatic nerves, E, of the spine, D, being raised by a glass rod, so as to touch the muscles of the upper frog at A, contractions are produced in each of the animals, they having been, in this case, as in the former, previously moistened with a strong solution of the muriate of soda.

If a similarly prepared frog be held by the hand, well moistened in salt and water, and the crural nerves be applied to the tip of the tongue of the person by whom the experiment is made, the convulsions will be produced.

From these nice experiments it has been deduced by physiologists, that, in the animal body, a principle exists, which may, by a recourse to certain particular means, be excited and directed at pleasure. They have besides conjectured that an energetic fluid is developed by the action of the animal forces.

I presume that these surprizing facts, the truth of which I have many times ascertained, are reducible to the principles I have already laid down; and that the galvanic phenomena are merely owing to the disengagement of the combined electricity of the substance acted on, while it undergoes a certain decomposition. In the case either of metallic evolutions, or of animal elicitations, it would appear that an oxydifying process ensues. When a galvanic combination of metals is placed under a receiver, a portion of pure air is lost. ALDINI suspended in a bell-glass fourteen frogs prepared with the utmost dispatch, and found that at the end of 24 hours the water had risen in the bell to the height of about half an inch.

This experiment would have been more satisfactory, if our naturalist had ascertained the loss of air relatively to the capacity of the receiver; and also whether the pure part of the air was thus abstracted.

This experiment of Professor MAJON, of Genoa, was made with a greater precision. He took a strong torpedo, and as soon as it was dead, armed its nerves with the usual coating. Having placed it on an insulating stool, a little elevated above water, he covered it with a bell-glass, the contents of which were equal to 432 cubic inches. At the end of some hours he observed, that the water under the receiver began to rise progressively, insomuch, that at the end of 48 hours it had risen above an inch, occupying a ninth part of the capacity of the bell, viz. forty-eight cubic inches.

The residual air was analyzed by the professor, who found that 80 cubic inches only of oxygen gas remained, with 324 of azotic gas. It thus appeared that, during the above period, more than two-fifths of the oxygen gas contained in the receiver, had been absorbed.

Animal substances, as conductors of electricity, have their capacities proportioned to their conducting powers. No change can be induced in these substances, without an adequate change in their capacities. Both the muscular fibre, and the nervous substance, in their healthy living state, and connected with the sources by which their energies are preserved, are, with respect to electricity, in a precise balance with each other. When a portion of animal substance is detached and separated, changes are induced, and decompositions effected. In cold-blooded animals, the process being very slow, the muscular fibres remain for a long time susceptible to so pervading a stimulus as electricity. When the substance is immersed in a solution of the muriate of soda, the nervous and muscular parts seem not to undergo equal changes. The part which is the most rapidly acted on, will leave the other in a negative state of electricity; and, on the communication being formed, convulsions will be produced. If the communication be made through the medium of a warm-blooded animal, similar effects will ensue.

As the changes* induced in animal substances, are by no means so considerable as in metallic bodies, relatively to the disengagement of electricity, unless the frog be very vigorous and lively, the experiment will not succeed. Female frogs are best calculated for these experiments; and should be chosen at the period when they emerge from their torpid state. The principle of life not having been excited during their sleeping season, is at this period in the least state of exhaustion.

From experiments made by Dr. FOWLER on leeches, it was conjectured by him, that galvanism is a principle independent of nervous influence, seeing that it acts on animals divested of nerves.

In the last century, POUPART described nerves in the leech; but MORAND and DILLENIUS denied their existence. WILLIS proved the existence of a brain in snails, notwithstanding LUDWIG denied them nerves. PRESCIANI and MANGILI have, however, demonstrated in them the existence of nerves; and this discovery ABILGOARD and RALJE have confirmed.

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* A curious experiment was tried by LA GRAVE, and related in the Memoirs of the Galvanic Society of Paris. A series of galvanic combinations having been formed of brain and muscle, cut into slices, and layers of hat, moistened in solution of the muriate of soda, evident galvanic effects were induced.

In animals which possess loco-motive powers, unless a connecting medium were diffused through their system, to the end that any part should be influenced at will, their motions would be irregular and undetermined, or, perhaps, would cease altogether. By this connecting medium is understood the nervous system; and, even if the nerves cannot be traced by the knife of the anatomist, it would be highly absurd to deny their existence on that account. Anatomy being the mere mechanical part of science, to believe no farther than mechanism can reach, would argue very limited ideas. The anatomist may be regarded as the pioneer of the physiologist, to clear away whatever may impede the latter in his researches. When the anatomist and physiologist are united in the same person, there must then be a proportionate efficiency of means. Animal substances are superior conductors to those which belong to the vegetable kingdom. The substances which constitute the intermediate link between animals and vegetables, such as mushrooms and morells, preserve the same order in their conducting powers. GEHLER, in his Philosophical Dictionary, first remarked this curious circumstance, which is not dependent on their humid state, since, when they are cut and dried, they are found to be good conductors. Mushrooms, when placed in a retort, and exposed to a lamp heat, give out water, which soon becomes putrescent, and evinces the existence of ammonia, of carbonic acid, and of an empyreumatic oil.

Morells contain a quantity of gelatine, and, owing to this enriching quality, constitute one of the luxuries of the table.

Fungi of this description, on the stimulus of light, do not, like vegetables, give out pure air, but evolve non-respirable gases, namely, hydrogen and the carbonic acid gas.

Vegetables in general are gradual in their growth; but these animalized vegetable substances spring up after one showery night.

Valli was the first to notice that a ligature applied to a nerve, prevents the muscle from being convulsed by metals. Trattori observed, that this is not an invariable consequence. Valli, in his subsequent observations, discovered that if the ligature be made close to the insertion of the nerve in the muscle, it entirely impedes its movements; but if at a distance, the experiment succeeds perfectly well.

However a nerve may be lacerated, if a small fibre of communication be left, the convulsions will be produced.

When the nerves of warm-blooded animals are applied to the nerves of cold-blooded animals, the contractions do not take place. Thus, the crural nerve of a mouse being applied to the crural nerve of a frog, and the nerves separated by about the half of a line, the contractions do not ensue. But if a nerve of the same frog be divided, and the parts separated about a line from each other, upon completing the circuit, the contractions will take place*.

A nerve, when divided, requires a considerable time after its subsequent union, to recover its original energy. ARNEMANN observes, that divided nerves unite by means of a cellular tissue formed by coagulable lymph; and notices also, that the voluntary powers of the muscles are restored, but not their sensibility†. The celebrated SOEMMERING's opinion is, that the reproduced portion never has the characteristic appearances of the other nerves. FONTANA and CRUICKSHANK have proved the fallacy of this opinion, by their experiments on the grand sympathetic nerve; and Dr. HAIGHTON's ingenious experiments fully prove the perfect re-establishment of the nerves in general. SPRENGEL, in an ingenious work on these organs, has referred our sympathetic sensations to the action of the nerves on the brain, and to the re-action of the brain on the nerves, in a state of unison. By others, these sensations have been ascribed to the anastomosing of the nervous fibrils; while others again have conjectured that each nerve has its particular atmosphere; and that, when these atmospheres are blended, the impressions are communicated*.

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* These experiments were made by HUMBOLDT.

† Versuch über di regeneration der nervem. Lib. i. cap. 3 and 5.

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* The infinite variety of the sensations we experience cannot be referred to the mere diversity of mechanical impulse: they are reducible to a few. Let us consider the nerves as ramifying all over the body, and that each branch is the trunk of the fibrils which proceed from it. When an impression is made on any part of the body, such an impression acts on a certain number of nerves. Now, if we suppose the impressions to be confined to so small a space as only to act upon two branches, the sum of the actions will be united in the trunk. This sum will be a varying quantity, according to the angles which these branches form with each other. The larger the angle, the less will be the sum of their united powers. When we consider that three, four, or five branches may be acted on, the infinity of results will enable us to conceive the infinitude of sensation upon the principles of the composition of forces.

The same principles are applicable to the varied visual sensations produced by the impressed optic fibrillæ; as well as to the great diversity of sounds from the undulatory actions on the auditory expansion.

I have been long engaged in physiological researches relative to nervous energy; and whenever my series of experiments shall be completed, I shall presume to submit them to the public.

CHAP. XXIX.

On the galvanometer—Mr. Pepys' *moveable coatings—Mr.* Read's *condenser, adapted to the electroscope, improved by* Cuthbertson*—The principle of action the same as in the Leyden phial—The state of electricity induced by a zinc, or by a copper-plate—The part of the cap the best adapted to the application of the metallic plates—Conjectures upon the cause of this difference.*

MR. PEPYS, of the Poultry, well known as a liberal and spirited philosopher, whose experiments are conducted with the utmost correctness and perspicuity, and whose observations and deductions are demonstrative of great ingenuity, has contrived a very curious apparatus, to be adapted to Bennet's electroscope, for the purpose of evincing small states of electricity, as they are developed in galvanic experiments. With this view, instead of the tin-foil coatings withinside the glass, he has substituted two plates, forming an acute angle, which, by a regulating screw, can be adjusted to any required distance from the gold leaves. The angular part is se-

cured

cured to the bottom; the open part perpendicularly upwards; and the gold leaves hang between them. By this ingenious mode of approximating the coatings to the gold leaves, the resistance being diminished, a weaker intensity of electricity suffices for their disturbance.

This contrivance will answer only when a series of galvanic combinations are employed; in the same way as Mr. READ's condenser, improved by Mr. CUTHBERTSON, evinces a disturbance upon the contact only of two dissimilar metals. Owing to its superior sensibility, it is now generally denominated the galvanometer.

Mr. READ, of Knightsbridge, well known by his valuable observations on atmospherical electricity, is the first who applied the apparatus, called the condenser, to the electroscope, to the end that it should evince small intensities of electricity. The very minute portion of the fluid given out by the single contact of two different metals, does not produce any disturbance of the gold leaves; but when several minute portions are accumulated, a separation of the leaves takes place. The electroscope, in its simple state, will be as much charged the first time as if the contact had been made a thousand times, and cannot therefore acquire a greater quantity of the fluid than suffices to place it *in equilibrio* with the metallic plates. This portion being inadequate to the production of any divergency of the leaves, Mr. READ applied the principle of the electrical doubler to the above instrument, by which means he was enabled to charge an intervening plate of air. By thus accumulating every minute portion of the fluid imparted by the metallic plate, and by apparently condensing and increasing its intensity, he ultimately succeeded in producing marked signs of disturbance.

This instrument has been considerably improved by Mr. CUTHBERTSON, as is the case with all those which pass through his hands.

The construction of this apparatus * is as follows:—A circular brass plate, *b*, *Fig*. 2, *Plate* V. about four inches in diameter, is soldered to the brass cap of BENNET's electroscope, *o*; and, at a right angle with the plane of the cap, a similar brass plate, *a*, connected with a brass rod, and moveable about the joint, *g*, completes the apparatus. The latter plate, *a*, is non-insulated, and is never in contact with the plate *b*, being placed at such a distance as to allow a thick card to pass through. Consequently there will be a plate of air between *a* and *b*; and the charging of this plate of air is precisely on the principles described in the explanation of the Leyden phial.

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* My worthy friend Mr. CARPUE, in his ingenious treatise on electricity and galvanism, has, by mistake, attributed the construction of this instrument to Mr. CUTHBERTSON. This gentleman, in the account he has published of the above contrivance, has, with his usual liberality, given every requisite credit to the abilities of Mr. READ.

Fig. 3, *Plate* VI. represents two metallic plates, A and B, about six inches in diameter, with insulating handles, as at C. One of these plates is made of zinc, and the other of copper*. When they are brought frequently into contact, and when, on each separation, one of them, for instance, the zinc plate, is, by its insulating handle, made to touch the cap of the electroscope, each portion of the electricity communicated to the cap, becomes equally diffused throughout the whole metallic medium in contact with it, the plate, *b*, participating of the effect. The surrounding air is so feebly acted on by this very small intensity, that unless its resistance be considerably diminished, no sensible effects will be produced. The very small plate of air between the two plates *a b*, becoming influenced by this weak state of electricity, the proportion of the fluid it receives on the side opposed to the plate *b*, it gives out at the side contiguous to the plate *a*, which, not being insulated, admits it with facility. In this way *a* may be considered as the coating of the outside of the jar, and *b*, as the inside coating. On every successive charge the plate of air becomes additionally acted upon. So long as the plates *a* and *b* are preserved in this state, no separation of the gold leaves takes place. When a Leyden phial is charged, and then placed on an insulating stand, if a communication be formed with the external coating and an electroscope, no disturbance will take place. The air in the external surface being in a precise balance with the air on the internal surface, no change can take place on one side so long as the other side is kept insulated. Thus it is with the condenser: the instant the plate *a* is separated from the plate *b*, the electricity which had been imparted and received by the intervening plate of air, being now exposed to the atmospheric mass, moves in the direction in which it meets with the least resistance. This direction is the metallic cap of the electroscope; and, according to the quantity thus regurgitating, the gold leaves become diverged. The state of electricity is always positive, if the zinc plate be applied; and negative, if the application of the copper-plate be made.

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* Care should be taken that brass be not substituted for copper, as the former metal contains zinc.

If either the zinc or the copper-plate, be applied to the part of the cap, *n*, which is directly opposite to the plate *b*, the effects will be stronger than when it is applied nearer to the plate *b*. This observation was first made to me by my very ingenious friend Mr. READ; and seems to accord with VOLTA's notion as to moto-electric power. It is a further confirmation of the idea, that the real conductors of electricity are replete with that principle; and that the first effects produced, are occasioned by the disturbance of the natural electricity of the body. It is well known to be a common property of all universally diffused fluids, to be more powerfully acted upon in the part which is in a line of direction with the impelling power. This is the case with the transmission of sound through solids or fluids, and also with the apparent motion of light *. The successive impulses acting at *n*, the natural electricity of the plate *b*, will also more powerfully disturb the intervening plate of air, than when the contact is made at any other part of the cap.

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* I say apparent motion, because I am induced to believe that light is universally diffused, and that its impressions are communicated by impulses. In all its properties it accords with those of universally diffused fluids. Its velocity is uniform, whether from Sirius or Jupiter. This is not the case with substance evolved from any particular bodies, the subsequent velocities being by no means equal to the initial force and impression. But water, air, &c. are uniform; and the laws of refraction and reflection are similarly dependent on the same mechanical principles.

In such a case, the motion communicated to the electricity of the plate *b*, will be weakened in proportion to the obliquity of the direction. This fact is easily proved, by the greater number of contacts requisite in one direction than in the other. With Mr. Read's apparatus I found that twelve contacts at *n*, produced a more sensible disturbance than twenty contacts did at the part of the cap distant one-fourth of its circumference from the plate *b*.

It would perhaps be more correct to call this instrument a galvaniscope, seeing that it does not so correctly measure the powers of a battery as one which will be hereafter mentioned, and which may therefore be more properly regarded as a galvanometer.

In the plate where this is represented, viz. *Fig*. 2, *Plate* V. *c* points to where the gold leaves are suspended, and *e* the tin-foil coating on the inside of the glass.

CHAP. XXX.

On the Voltaic accumulation—The couronne de tasses*—The pile—*Cruickshank's *trough—The superior advantages of this construction—Method of preparing galvanic batteries—What liquids are the most convenient—Order to be observed in connecting two or more batteries.*

WHEN Galvani's important discovery was communicated to the world, it was necessarily expected that it would open a new field of inquiry, relatively to the principle of action in animated bodies. The interesting experiments of Bertholon had already proved the energetic influence of electricity on the vegetable kingdom; and the curious memoir of Thouvenel, on the *virgula divina* of Bleton, had evinced the susceptibility of the animal frame to the influence of minute portions of the same principle.

Notwithstanding the experiments with dissimilar metals had been diversified by naturalists of different countries, and although the fact first noticed by Sultzer, of their influence on the organ of taste, had been confirmed by the ingenious experiments of Volta and Robison, who had likewise noticed the more remarkable effect of the luminous appearance first observed by Dr. HUNTER, of York, produced by their contact, still nothing more than a temporary interest had been excited. As the results did not appear applicable to the explanation of any physical phenomena, they were gradually lost sight of; and in spite of the intelligent observations of CREVE, FABRONI, and ASHE, in a few years after its discovery, galvanism sunk in a manner into oblivion.

Hitherto physiologists had turned their attention entirely to the disturbance of the muscular fibre. In increasing the scale of their experiments, they confined themselves altogether to the augmentation of the animal substances employed. With this view batteries of frogs were arranged; but no idea seems to have been entertained of what was to be expected from an increase of the series of metals.

VOLTA, whose name is so celebrated in the annals of philosophy, is the author of this discovery, which he communicated about three years ago in a letter to Sir JOSEPH BANKS. He there observes, that a series of this nature resembles an electric battery, weakly charged, acting incessantly, and which charges itself after each explosion. Instead of consisting of insulating plates or electric strata, it is armed with conductors.

The first arrangement he describes, is what he terms a *couronne de tasses*, consisting of glass tumblers, half filled with water, or salt and water, and forming a regular union by the series of metallic arcs represented in *Fig.* 5, *Plate* VI. in which the plates marked S are either of silver or copper, and those marked Z of zinc. The arcs *a*, *a*, *a*, may be formed of any substance which is a good conductor of electricity; and the two metals in each glass should be soldered to the metallic arc, in any part above the one which is immersed in the liquid. VOLTA remarks, that alkaline solutions are employed with the best effect, when one of the metals is tin, and the other silver or copper. If zinc be substituted for tin, then salt water is preferable.

When thirty or forty of these glasses are arranged, one of the experimenter's hands being placed in the fluid contained in the first glass, and the other hand in the last in the series, a shock is experienced. As often as the circuit is completed, the same sensations recur.

VOLTA next communicated a more convenient mode, by substituting wet pieces of cloth to the glass tumblers, as in *Fig.* 1, *Plate* VII. which represents a frame formed of two circular pieces of wood, secured to columns of varnished wood or of glass, *c*, *e*, *d*. At the bottom of the frame, at *f*, a plate of zinc, soldered to a plate of copper, is first laid down with its zinc side uppermost. Upon this a piece of cloth, of a somewhat smaller circumference than the metal, and well moistened with salt and water, is placed. Next, follows another plate of zinc and copper, the same arrangement as at first being observed, namely, that the zinc side should be uppermost. Then a second piece of wet cloth, &c. By thus continuing the series to 40 or 50 plates, a galvanic pile will be constituted. On placing one of the hands at *f*, and the other at *g*, a shock is felt. This pile Volta terms a moto-electric apparatus: the process continues as long as the pieces or discs of cloth remain moist; as soon as they become dry the operation ceases.

To preserve their action longer, Volta suggested a mode of enveloping a pile with cement, without any interposition of cloths. He proposed to station the metallic plates at a greater distance from each other, and to fill up the cells, or intervals, between them with a saline solution.

Mr. Cruickshank, the celebrated chemist at Woolwich, considerably improved this Voltaic construction, after the manner represented in *Fig*. 2, *Plate* VII. A B C D is a wooden trough formed of hard baked mahogany; about thirty

inches

inches long. Withinside, fifty grooves are cut in the sides and bottom, at equal distances from each other. These grooves should be of a thickness corresponding to the metallic plates*. The plates are cemented separately in each of the grooves, so that the fluid will not pass out of one cell into the other. The plates consist of zinc soldered to copper; and in whatever direction the first of them is cemented into the box, all the others must preserve the same order. Supposing the copper side of the first to be towards

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* The mode of forming these metallic plates is as follows: The zinc, or, which is the same, the spelter of the shops, should be melted in a vessel which exposes but a small surface: it would otherwise absorb oxygen so rapidly, as to be volatilized, and converted into a fleecy substance, denominated the flowers of zinc. A stone mould about four inches square, and about one eighth of an inch thick, is well suited to this purpose; or a still better one may be formed of brass, by any founder to whom a pattern in wood is given. The plates should be cast as quickly as possible. Those of copper need not be more than one tenth the thickness of the zinc plates. The pieces of copper being cut of a size corresponding to the zinc, and hammered straight, so as to apply correctly to the zinc, the plates should be soldered together by means of soft solder, not through their whole extent, but about a fourth of an inch from the edge, so that at the edges the union may be complete.

The cement I generally employ, is made of four ounces of yellow wax, eight ounces of resin, and about an ounce of fine brick-dust.

B; all the others should be so placed in the box as to have their copper sides towards B, and their zinc surfaces towards C D. When they are thus arranged, the end C D is called the zinc end of the battery; and the end A B the copper end. In this state the battery is complete.

This construction possesses many advantages which the pile does not. The fluid is applied with greater facility. The apparatus is more convenient for experiments; it continues to be active considerably longer; and is attended with little or no trouble in cleaning. In the Voltaic pile, after it has been once used, the zinc surface of the plates becomes oxydated; insomuch, that before they can again be used, they must be scraped or filed, which is a very tedious and troublesome operation. In the trough every successive operation cleans the plates, by dissolving the oxydated surface of the zinc.

Volta supposed the fluid employed to act merely as a conductor; and that the differences in fluids depend entirely on their different conducting powers. It has already been shewn, by the experiments of Ashe, Creve, Fabroni, and others, that the fluid acts chemically on the easiest oxydable metal; and that those fluids produce the most active effects, which influence the metallic substance the most powerfully. Thus acids, alkalies, neutral salts, &c. act more powerfully than simple water. Acid solutions are still more powerful than the former. One part of nitric acid, blended with about twenty parts of water, forms a very active mixture. As this mixture evolves nitrous gas, it is injurious to respiration; besides which, the nitric acid is very expensive. Sulphuric acid, combined with water, answers very well, but its action is too quick. In operating on the zinc, it disengages such quantities of hydrogen gas, as to be very troublesome, and is frequently attended with such evolutions of heat, as to loosen the cement. About an ounce of the muriatic acid, or spirit of salt of the shops, blended with a pint of water, constitutes the mixture I generally employ. It acts tranquilly and uniformly, decomposing the water so slowly, that the hydrogen gas which is evolved, does not annoy the operator. The trough, by the employment of this acid mixture, is kept uniformly clean.

When the experiments are finished, a mixture of the above description should not be thrown away, but emptied into an earthen pan, and reserved for any subsequent experiment. For medical purposes I employ the same mixture eight or ten times. The trough, when emptied, should be rinsed out with fresh water, and will be then ready for use.

Some persons employ solutions of the muriate of soda, of the muriate of ammonia, or of the sulphat of potash. These solutions are comparatively weaker than the acids; and, by crystalizing on the plates, render the trough extremely difficult to clean.

When, for any experiment, several batteries are requisite instead of one, they should be placed in precisely the same order as if two, three, or more batteries were to constitute one trough only; invariably observing, throughout the whole of the series, however extensive it may be, that the zinc surface of one of the plates should be constantly opposite to the copper surface of the next plate in the series; and that the troughs should be connected to each other by any metallic substance. A slip of copper sheeting, about half the width of the trough, answers this purpose very well.

It is surprizing, that on the Continent the Voltaic pile should be still employed. To render its action more permanent, a Mons. Isam has substituted to the moistened cloths, salt just sufficiently moistened to be formed into a layer. He observes, that a pile thus formed, retains its powers for a month. The quantity of metallic substance destroyed by the acid is very trivial; since it only acts when the circuit is completed. A trough, I should presume, will, with due care, last ten or a dozen years, if it even be employed several hours daily. The fluid, when poured in, should not ascend higher than about a fourth of an inch from the top of the plates; and when the trough is filled, it should, as well as the upper surface of the plates, be wiped dry*.

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* When a single metal is employed, such as copper, the plates are cemented in the same manner: in one of the cells simple water is introduced; and in the other a solution of nitric acid. This battery is by no means so powerful as when zinc is employed with copper, and one fluid only. So, likewise, if plates of gold and silver be soldered together, a solution of nitric acid will be found to answer, because it will oxydate the silver, and not the gold. Any metallic combinations may be formed, if the fluid which is employed will act on one of the metals, and not on the other: their powers will, however, be feeble, because their changes are trivial when compared to those of a combination of silver or copper with zinc. The latter of these metals oxydates so readily, as to give out its combined electricity with greater facility than any other metal. In an extensive battery silver plates would become expensive, and as copper only loses one fifth of the power, or, in other words, as 80 plates of silver and zinc are equal in power to about 100 plates of copper and zinc, the difference is amply compensated by the saving which is made.

CHAP. XXXI.

How far the powers of galvanism are increased by the extent of metallic surface—The experiments of Fourcroy, Vauquelin *and* Thenard—*The shock is in proportion to the series, and not to the surface—Its powers in the deflagration of metals depend on the surfaces—Conjectures as to the cause—How far the surface is concerned, illustrated by experiments with six hundred small plates—The principles of the battery explained by the doctrine laid down in the chapter on electricity—The necessity of the cells being insulated—In a series of batteries, they should all be of equal powers and surfaces.*

SOON after the important communication of Volta on the self-charging power of his moto-electric apparatus, the celebrated French chemists, Fourcroy, Vauquelin and Thenard, supposing, *a priori*, that the effects would be in proportion to the metallic surfaces exposed, constructed plates of zinc and copper about nine or ten inches in diameter. The plates which had been previously employed, rarely exceeded the size of half-a-crown. As one of these large plates exposed a surface about 40 times* greater than that of the small plates, it was expected that the effects produced from ten such plates would have equalled the effects of four hundred of the smaller ones; but, to their great surprize, the above experimenters found that the sensations were the same, from an equal number of each; and that the extent of surface did not make any sensible difference. The power appeared to be regulated by the number of the plates, and not to be influenced by the surfaces exposed. They were equally surprized, when they found that a series of ten large plates, the effects of which on the human frame could with difficulty be perceived, possessed a power of burning finely drawn wires, and of deflagrating laminated metals, such as gold and silver leaves, equal to that of four hundred smaller plates, the sum of whose surfaces was equal to the surface of the larger ones. These curious facts led to an important deduction, that the influence of the galvanic fluid on the human frame, is in proportion to the series, or number of the plates employed;

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* A half-crown measures about $1\frac{1}{2}$ inch in diameter, and the diameters of the large plates being estimated at 10 inches, as the areas of circles are, in proportion to each other, as the squares of their diameters, the ratio will be as 100 are to $2\frac{1}{4}$, or as 1 is to 40.

ployed; while its effects on metals are in the ratio of the surfaces exposed.

In the course of my inquiries*, I have not heard of any attempt to explain these remarkable and curious circumstances. As such inquiries appear to me to be of considerable importance in this branch of natural philosophy, I shall presume to suggest such explanatory conjectures as have occurred to me.

The quantity of electricity evolved from each plate I have already shewn to be so extremely minute, as to require an accumulation before the most sensible artificial electroscope can be disturbed. To conduct the portion of electricity which is thus disengaged, such media must be employed as occasion the least resistance to its passage. It is well known to electricians, how extremely difficult it is to liberate a jar once charged, of every portion of electricity: after the metallic circuit has been completed five or six times, a sufficient proportion remains to produce a disturbance in the galvanometer.

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* In the French Memoirs several ingenious remarks are made by Biot, on the surfaces of plates. He observes, that the circumstance of small plates giving equal shocks with large ones, depends on the velocity. If there be a less mass of fluid, the velocity will be greater; if a greater mass, it will be less. As shocks depend on the velocity of the fluid, they will not be increased by the size of the plates; while metallic combustion will depend on its mass. I presume that, from the experiments I have adduced, this theory of Biot will be found incorrect, as to any difference of velocity. It will indeed be the reverse of what Biot has asserted.

If, instead of a metallic circuit from the inside to the outside of the jar, the human body be substituted, it becomes almost impossible to perfectly equalize the jar. Mr. CAVENDISH has demonstrated, that the conducting powers of water * for electricity, are one million times less than the powers of metallic bodies; or, which is the same, that the resisting power of water to the passage of electricity, is one million times greater than the resistance from metallic bodies.

A battery, discharged through an interrupted circuit in a tube of water, will burst the tube; while the same charge will be tranquilly conducted by a fine metallic wire.

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* Mr. CAVENDISH shewed, by a very ingenious experiment, that a cubic foot of water is merely capable of transmitting a full discharge from a jar of one foot of coated surface.

In the Philosophical Transactions for 1776, Mr. CAVENDISH observed that a piece of iron wire, 400 millions of inches in length, resists the passage of electricity no more than one inch of water, of the same cylindrical dimensions as one inch of the iron wire. By what experiments this celebrated philosopher was led to the above conclusion, I am ignorant; in the paper in question he says, that the conducting powers of iron to water are as four hundred millions to one.

The human body being constituted of conducting media nearly equal to water, must therefore considerably resist the passage of electricity. As the effects can never be greater than the cause, such an intensity of electricity must remain on the plates as to be equal to the resistance through the body. In such weak accumulations as occur in galvanic operations, little more can be transmitted through the body, than a portion of the fluid with which it is in contact. If two fingers be employed, instead of one, the sensations will be greater. In a trough consisting of a series of large plates, if three or four persons form as many circuits, from the zinc to the copper end of the battery, very little difference will be experienced by an additional person who at this period receives a shock.

If a small metallic wire be extended from one end of the trough to the other, such a communication will effectually prevent any sensations being induced by the application of the hands. The conducting powers of metals are so superior to those of animal bodies, as to have, comparatively, a small portion of the fluid on the plates; and hence what they transmit will be in proportion to the surfaces. But so imperfect a conductor as the human frame, transmitting merely the portion with which it is in contact, can then only convey what is more than a balance for its resistance*; all the plates possessing equal intensities, will give out equal quantities, therefore the sensation will be nearly in proportion to the series and not the surface.

Some idea may be formed of the great degree of resistance, in the passage of the galvanic fluid through animal substances, by the following fact; that if, in the case of a powerful accumulation of three or four hundred plates, five or six persons form a circuit, the sensation is very trivial, when compared to the one experienced by a single person by whom the circuit is formed.

As I could not observe any sensible difference in sensation, from a battery consisting of 50 square plates, the sides of the squares measuring each two inches, and from a battery of the same number of plates, the sides of which measured four inches, notwithstanding each of the plates of the latter exposed a surface four times greater than that of the plates of the former, I was induced to suppose that the same rule would hold good with batteries constituted of still smaller plates.

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* Supposing the intensity of electricity to be equal to ten, and the resistance of the body equal to 6, it is evident that 4 will express the quantity transmitted. If the resistance of a metallic substance be supposed equal to 1 only, then the quantity transmitted will be equal to 9.

To determine this, I constructed a battery of six hundred plates, each measuring on one of its sides only three-fourths of an inch*, and exposing a surface of very little more than the half of a square inch. The shock from two hundred of these plates was violent; but not quite equal to that of the same series of larger plates. After the first shock, the second was very feeble; the third still fainter; and so on until the tenth, which was hardly discernible. A circuit formed with the whole of the battery was at first so extremely violent as to produce a very unpleasant sensation; but every successive communication became weaker and weaker, till about the tenth shock, when it was so faint as scarcely to be felt.

This I found to be invariably the case when the communications were made rapidly; but when I left an interval of about five minutes, every succeeding shock was equally violent.

These curious facts will, I presume, tend to the elucidation of the reasoning above stated.

When a large metallic surface is acted on, upon the abstractions of a certain portion of electricity, the remaining quantity becoming equally diffused, the intensity at that instant, supposing no more to be decomposed, will be in the proportion of the remaining quantity to the quantity which existed previously to the abstraction.

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* The plates measured one-fourth less when cemented, from a portion of the surface being covered with the cement employed: the measure given, is what was exposed to the action of the fluid.

Supposing the quantity of electricity on the surface of a large plate to be estimated at one hundred thousand, and the quantity abstracted by the contact of an animal body to be equal to one hundred, it is evident that the intensity of the electricity on the plate, after the abstraction, will be lessened by one thousandth part only. A plate having a tenth part of the dimensions of the former, will have a quantity equal to ten thousand; and, after the abstraction, its intensity will be diminished one hundredth part only. In either case, the constant decomposing process will supply the deficiency; insomuch, that the subsequent actions will appear uniformly equal from a plate of either size.

If the metallic plate have only the hundredth part of the size, the quantity of electricity will, in the same proportion, be expressed by a thousand. One hundred particles of electricity being abstracted, the intensity on the plate will be diminished one tenth. The oxydifying process, in going on, requires, to supply this deficiency, a time which is not required in the case of the larger plates.

From the above observations it will appear, that there are certain limits relative to the size of the plates; and that the oxydifying process is a very gradual operation *.

As the cells of a battery, or the interposed discs of a pile, are replete with fluids, which are good conductors of electricity, it was presumable, on an early view of the subject, that an equalization would take place.

That this does not occur, has been conjectured by several experimenters, to arise from the imperfection of the conducting medium. This is not, however, the cause; and on a strict inquiry, we shall find the fact to be consonant to common electric charges. Suppose *Fig.* 4, *Plate* VII. to represent a small battery, and 1, 2, 3, to constitute the cells: if the inequality were owing to the imperfection of the conducting medium, this would be ascertained by the interposition of a metallic substance connecting one plate with another. With this view 1, 2, 3, are made to represent pieces of thick copper wire, placed from one plate to another. With this conducting medium, the best we have, the charge is, however, in no degree diminished.

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* It is upon this principle, that a battery consisting of small plates is more readily oxydated than a series of large plates, although under equal operations.

The establishment of a communication between the zinc surface of one plate, and the copper surface of the next, cannot, upon the principles of charged jars, produce an equalization.

A simple galvanic combination is to every purpose and intent a Leyden jar; and the Voltaic accumulation, the battery.

Instead of two metallic plates, let us substitute two Leyden phials; and we shall find the results precisely the same. Let these two jars be charged, afterwards disengaged from the prime conductor, and then placed on an insulating stand. When they are in this situation, on a communication being formed from the outside of one jar to the inside of the other, the electrical effects will not ensue, although a communication is formed between the sides of two jars, in contrary states of electricity. Consonant to the theory I have already laid down, the charges of a jar take place in the portion of air alone which is contiguous to the surface of the glass; the columns withinside consisting of electricity without air, and the columns contiguous to the outside coating being constituted of air without electricity. When these columns are in the above state, in consequence of their reciprocal actions and re-actions, a change cannot be induced on one of the sides, unless by a communication with the other side. To produce a discharge, a circuit must be formed from the inside to the outside of the same identical jar; and not to the outside of another jar. I should presume that the application of this theory to the explanation of the phenomena of the galvanic plates, is very obvious. To produce an equalization, it is necessary that a communication should be formed between the copper and zinc surfaces of the same plate. In effecting this, it is not necessary that the conducting medium which forms the circuit, should be in actual contact with the copper and zinc surfaces: it suffices that it should enter into the liquid contiguous to each of the surfaces. This is an additional proof that the defect of equalization does not depend on the imperfection of the conducting medium. When a diluted acid is employed, as the fluid, its conducting powers are very superior to those of water.

In discharging a galvanic battery, the sensation will be in proportion to the number of plates between the points of contact. We will suppose the trough A B C D, *Fig.* 2, *Plate* VII. to consist of sixty plates. If, between B and 1, there are fifteen plates; and between 1 and 2; 2 and 3; and 3 and C, the same number; and if one of the hands be applied at B, and the other at C, the whole force of the battery will be experienced. But if one of the hands be applied at B, and the other at 1, only one fourth of the shock will be felt; and it is consequently in proportion to the number of the plates between the two hands. In this way a given power of electricity is divided into any required minute portions, with an accuracy which cannot be imitated on the common electrical apparatus.

When a communication is formed between B and C, it appears, at a first view, to controvert what has been already stated, relatively to the necessity of connecting the zinc side and the copper side of the same identical plate, in order to produce a discharge; seeing that the charges of all the plates are thus communicated. Upon a little reflection, it will, however, be obvious, that this is a necessary result, and a powerful confirmation of the doctrine already laid down. When one of the hands is applied at B, supposing the latter to be what is termed the copper part of the trough, and the end C the zinc part, the hand at B is not only in contact with the copper surface of the first plate, but, on account of the regular chain of good conducting media, is also connected with the copper surfaces of the other plates. In the same way the hand at C, which touches the zinc surface of the same plate, is, by a similar channel of communication, in conducting union with the zinc surfaces of all the plates towards B. Let us suppose the plates alone, from B to 1, to be under the galvanic action; and the cells from 1 to C to be filled with pieces of charcoal: upon forming the communication between B and C, the united powers of fifteen plates will be transmitted. Now, the plates from 1 to C, in this case, act merely as conducting media. It is precisely the same when all the cells are filled with an acid solution. This fluid keeps up the connexion of the chain from plate to plate; and they then reciprocally act as conducting media to each other.

From these observations may be perceived the necessity of the cells being insulated in such a way as that the fluid cannot pass from one to the other. If such a communication should exist, there would then be a connecting medium from the copper surface to the zinc surface of the same plate. On this account it is necessary that each plate should be well cemented in the apparatus. When a battery is carelessly made, the appearance of the plates will evince the non-insulation of the cells. As was before observed, the oxydifying process can only continue in proportion as the decomposed electricity is disengaged. Thus the plates which constantly have a connecting and conducting medium from the copper surface to the zinc surface, are rapidly oxydated; while the other plates merely undergo changes, at each of the instants when the circuits are completed.

In combining several batteries*, care should be taken that they be nearly of equal force. If an imperfect battery be interposed, the united power of the whole will be merely equal to the same series of the imperfect one. In processes of deflagration, a battery † consisting of small plates should not be ranged with a battery having large plates, the effects of the latter being diminished considerably by the former.

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* If an imperfect battery, consisting of fifty plates, be united with five good batteries having the same number of plates, the effect of the whole will simply be equal to six times the power of the imperfect battery. Supposing the imperfection to arise from the non-insulation of the cells, the same circumstances which occasion its own charge to be weakened, will proportionably prevent the transmission of higher charges from another battery.

† If a battery of plates, having a surface of thirty-six inches, be connected with another battery of plates having a surface of one hundred inches, their combined effects will differ but little from the proportion of their surfaces. But if to these two batteries, another with plates, having an inch only of surface, be added, the power of the whole will be diminished. It is the same with electricity. If three jars of different sizes be charged, one exposing a surface of coating of a foot in dimension, another of half a foot, and the third of one-fourth of a foot, their united powers will be very little more than would result from thrice the surface of the smaller jar.

CHAP. XXXII.

On the decomposition of water, by Messrs. CARLISLE *and* NICHOLSON—RICHTER'*s experiments to shew that the effects are contrary to the theory of* LAVOISIER—*Remarks on the inaccuracy of the above experiments—Dr.* GIBBES' *new theory —Dr.* WOLLASTON *and Dr.* PEARSON *on the decomposition of water by electricity—On the decomposition of inflammable fluids--Remarks on the decomposition of water—Conjectures on this operation.*

IT has been frequently remarked, that Great Britain has given birth to very few philosophical discoveries; and it has been conjectured that its inhabitants do not possess the lively and inventive imagination necessary to their production. Although France can boast a LALANDE, a LAPLACE, and a LAGRANGE, as well as a CHAPTAL, a BERTHOLLET, a GUYTON, a FOURCROY, and a VAUQUELIN; and notwithstanding Italy has reason to be proud of FONTANA, VOLTA and GALVANI; still I presume that we shall not sink in scientific estimation, while we can reckon such names as those of a CAVENDISH, a PRIESTLEY, a KIRa KIRWAN, and a BANKS. Whatever superiority of invention the continental philosophers may claim, science is, in general, indebted to our experimenters, for the perfection of the discoveries which have from time to time been made.

Relatively to galvanism, which is become a new branch of natural philosophy, the labours of the British philosophers have been eminently distinguished. The earlier periods of galvanic researches were enriched by the valuable observations of MONRO, FOWLER, ROBISON *, WELLS and HUNTER; and since the discovery of the Voltaic pile, Messrs. NICHOLSON, CARLISLE, WOLLASTON, CRUICKSHANK, DAVY, PEPYS, &c. have devoted to this science considerable attention; insomuch, that their inquiries have been disseminated through every civilized country, by the highly respectable media of NICHOLSON'S Journal, and TILLOCH'S Philosophical Magazine.

Soon after VOLTA'S communication to Sir JOSEPH BANKS, that distinguished patron of the sciences transmitted the particulars to Messrs. CARLISLE and NICHOLSON, from whose scienti-

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* This gentleman ought not to be confounded with a Frenchman of the name of ROBERTSON, who has exhibited a large Voltaic pile in Paris, and by whom the original phantasmagoria was exhibited in that capital.

fic skill he had reason to expect that the experiments of VOLTA would be repeated with every requisite attention. Upon the pile being constructed, and the communication formed, Mr. CARLISLE perceived that several bubbles of air were disengaged from the moisture on the top of the pile. From the smell Mr. NICHOLSON conjectured it to be hydrogen gas; and that, during the process, water had been decomposed. It had been asserted by CREVE, four or five years before, that in every galvanic operation water is decomposed; but this was merely conjecture, unsupported by any proof, and cannot therefore have diminished the merit of the real discovery. It occurred to Mr. NICHOLSON, that if the shock were to be transmitted through an interrupted circuit in water, the fact of the decomposition would be ascertained. With this view he constructed an apparatus similar to the one described by *Fig.* 3, *Plate* VII. in which A B represents the Voltaic pile, and C D a glass tube filled with water, a wire proceeding from the upper part of the pile into the tube at C, and ending at *e*, while another wire passes from the bottom of the pile through the end D towards *f*, leaving an interruption, *e*, *f*. The apparatus having been thus arranged, one of the wires was observed rapidly to give out a gas, which, on examination, proved to be hydrogen; while the other became oxydated.

dated. When wires of gold or platina were employed, these being non-oxydable metals, oxygen was, as well as the hydrogen gas, given out. It was invariably remarked, that the wire connected with the zinc end of the battery gave out oxygen, and the other wire hydrogen gas *.

When the above curious experiments were repeated in Germany, by Richter and others, it having been observed that a decomposition of the water took place, even when the wires were at a distance from each other, a doubt was entertained relatively to the accuracy of the decomposition. When the wires are separated by a space of two inches even, air is given out freely at both of them, the one giving out hydrogen gas,

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* I find the arrangement represented by *Fig.* 2, *Plate* VII. to be more convenient. A wire, *r*, which slides on one of the plates, supports a brass cup, in the centre of which is a pair of brass pincers holding a piece of gold wire; over these pincers a glass tube is placed, having at its extremity, *o*, a piece of gold wire sealed in with a glass, which should not touch the wire at *p*. The tube and brass cup being filled with water, a wire, *o n m*, connected with the top of the glass tube, joins the other end of the battery, by which means the circuit is completed and the decomposition goes on. As the gases are disengaged, they rise up to the top of the glass tube, and, pressing the water into the cup, the process will go on till the whole of it is pressed beneath the upper wire. The operation then ceases.

gas, and the other oxygen gas. As water is supposed to consist of oxygen and hydrogen, it becomes difficult to conceive how one and the same identical particle of water can be acted upon by the two wires at one and the same instant. One component part of a particle of water cannot be disengaged without the other component part being developed; and had this conjecture proved to be founded, it would have militated against the Lavoisierean theory. RICHTER, on a supposition that the same identical particle of water is not acted upon by both wires, constructed an apparatus with a view of proving, that by having recourse to two separate portions of water, one could be so arranged as to give out hydrogen without oxygen, and the other, oxygen without hydrogen; and that consequently this would be a demonstrative proof that the same identical particle of water is not decomposed by the two wires.

With this view he arranged an apparatus similar to *Fig.* 2, *Plate* XI. A B C being a curved glass tube placed in a glass vessel of water, A, and E two small glass bells suspended on a rod, Z C. These bells are filled with water, and placed over the aperture B C of the tube; at B a gold wire enters the tube, and has its outward end connected with the zinc end of the battery. At C is a similar wire in union with the copper end of the battery. The part of the tube A, is filled with sealing wax, through which runs a small gold wire. When the apparatus has been thus arranged, RICHTER asserts, that in the bell A there will be hydrogen, without the other component part of the water; and in the bell E, oxygen gas without hydrogen *.

To ascertain this point correctly, I placed at the zinc and copper extremities of a large battery, as at *Fig*. 6, *Plate* VI. two glasses for the decomposition of water, such as are described at the commencement of this chapter. By connecting the upper gold wires only, in the tubes *a b*, with the wire *c d*, the principles of the apparatus being precisely the same as those of the one described by RICHTER, in this case the gold wires in each of the tubes of water, were observed to give out gases freely; but when so much air was given out as to press the water below the upper gold wire, the operation ceased. The battery I employed consisted of sixty plates, each exposing a surface of sixteen square inches. Upon disengaging the wire from *b*, by applying it to the last plate in the battery at N, and moving the glass *a* till the two gold wires were brought nearly into contact, a luminous spark was given out, and an explosion ensued. On connecting the wire *c d*, with the gold wire at *b*, and, detaching the other end from *a*, applying it in a similar manner to the last plate towards M, an explosion also took place at *b*. To succeed in this experiment the gold wires in the pincers are so raised up, that upon moving the glass tubes backward and forward they may be brought into contact with the upper gold wires.

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* This is not precisely the apparatus RICHTER employed; but the principles are the same.

These explosions demonstrate the existence of both hydrogen and oxygen. By the above mode, the elastic fluid which is produced is never disturbed from the tubes. When a weak pile is employed, the wire connected with the zinc end of the battery is scarcely perceived to give out any gas. To this circumstance I attribute Richter's error. Independently of this, the test to which he subjected the generated air was very inaccurate. I am not aware of any experiment in which hydrogen gas is given out, as a component part of water, without it being possible also to detect oxygen.

The decomposition of water by electricity *, according to the mode suggested by VAN MARUM, is a very tedious process. Dr. WOLLASTON,

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* Dr. Pearson supposes the decomposition of water by electricity to be effected by the interposition of the dense electric fire, between the constituent elements of the water, which he

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he places beyond the sphere of attraction for each other, each ultimate particle of oxygen and hydrogen uniting with a determinate quantity of the electric fire, to bestow on them their gaseous form. Hence the Doctor supposes that the electric fire, after effecting the disunion, assumes the state of caloric.

On the reproduction of water by the passage of an electric spark through a proportionate quantity of oxygen and hydrogen gases, Dr. PEARSON ingeniously conjectures, that by the influence of the electric flame, the ultimate particles of these gases, the nearest to the flame, are driven from it in all directions, so as to be brought within the sphere of each others attractions. In one of these cases, Dr. PEARSON supposes that the caloric destroys the attraction, which in the other instance it occasions.

It is with diffidence that I take on me to controvert the opinions of this very respectable physician; but I presume that the whole of the phenomena of the synthesis and analysis of water, are more readily to be explained on the principles I have laid down, than by the adoption of the mysterious terms of attraction and repulsion. By the operation of galvanism, water is more rapidly decomposed than by common electricity. In this operation there is no evolution of dense electrical fire; but merely a current of a small intensity of electricity acting permanently and incessantly. To reproduce water, a flame must be generated sufficient to kindle the contiguous portion of the hydrogen gas, then the next portion, and so on, the combustion being preserved by the presence of the oxygen gas. As these processes proceed with immense rapidity, as soon as the gases are intermixed, so as to appear like one sudden explosion, the caloric of each of them being thus disengaged, their bases unite, and constitute water.

by an ingenious contrivance, and by the means of very small gold wires, has succeeded in decomposing water by the electrical machine, nearly in the same manner as is effected by galvanism. VAN MARUM's method is by an interrupted explosion; while in the case of Dr. WOLLASTON's, the production is tranquil and progressive.

The decomposition of water by galvanism, is of great use in the elucidation of its decomposition by electricity, which has excited so much the attention of philosophers. It has appeared difficult to comprehend how an electrical explosion should effect a decomposition of water; and, when it has been decomposed, should occasion its re-composition.

By the galvanic operation, we find this to be occasioned by the passage of a constant current of a small intensity of electricity, passing through an imperfectly conducting medium. The resistance it meets with occasions the separation of the constituent parts of the water, the actual substance of which is pervaded by this subtile principle, which thus breaks the bond of union of its constituent parts.

When these gases are inflamed by the electric spark, it is not the electricity which directly occasions the explosion; it being produced by the caloric elicited* from other substances, by the interrupted passage of the electric fluid.

When water is decomposed by the process described by VAN MARUM, the effect is occasioned by the passage of the electricity. The spark may be considered as a casual attendant.

It is a curious and remarkable fact, that inflammable fluids, such as oil, alcohol, &c. cannot be decomposed by the galvanic process, unless by the medium of the passage of a spark. Thus, in *Fig*. 4, *Plate* VI. supposing C D to represent a glass vessel half filled with alcohol; let A B be two pieces of well prepared charcoal, one of them connected, by means of a wire, with the zinc end of a powerful battery, and the other piece joined in the same way to the copper extremity. As often as the ends E F, which are immersed in the alcohol, are brought into contact, a brilliant spark is produced, and bubbles of air are given out. Upon continuing the process, provided the battery be very powerful, the spirits will almost boil. The air, which is rapidly disengaged, when collected in a receiver, proves to consist of carbonic acid gas, and hydro-carbonate gas. As alcohol is composed of oxygen, hydrogen, and carbon, at the instant of decomposition new combinations take place. The nascent hydrogen, uniting with the carbon, constitutes the hydro-carbonate; and the remaining portion of carbon, blended with the oxygen, forms the carbonic acid. The same results follow the decomposition of oil *.

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* Electricity, when it breaks through resisting media, produces an explosion proportionate to the resistance it overcomes. It disengages light and caloric from the body through which it passes; and the caloric seems to be disengaged, in the same way as it is liberated by bodies in a state of combustion. Electricity will however inflame warm spirits, which a heated body cannot do.

The decomposition of water by galvanism is governed by the laws which apply to animal substances. The rapidity of the decomposition is in proportion to the number of the plates, and not

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* VAN MARUM succeeded in decomposing various substances by electricity, after its intensity had been considerably diminished by its passage through an exhausted vessel.

When a spark of electricity, sent through a vacuum, is made to strike on water previously divested of its atmospheric air by boiling, air is abundantly given out.

When alcohol is subjected to this process, an air which proves to be hydrogen gas, is rapidly given out. On caustic ammonia being employed, a column of air, of twenty-one inches, was produced in five minutes. The ammonia gave out hydrogen and azote. The air produced from the water was hydrogen. In concentrated sulphuric acid no change was induced. The carbonate of potash did not give out any air; but the carbonate of ammonia gave out gas rapidly: solutions of silver, copper, iron, lead and quicksilver in nitrous acid, and of gold in aqua regia, did not afford any precipitation when a platina wire was employed.

to the surface; till we reach the limits particularized in speaking of the influence of small plates. In the battery of 600 small plates before described, the decomposition is by no means equal to a battery of 60 plates, each of which has a surface of four inches.

Dr. Gibbes, of Bath, a gentleman of considerable scientific acquirements, having deduced from the experiments of Richter, that the theory which makes the component part of water to consist of oxygen and hydrogen is incorrect, submitted to the Royal Society a very ingenious hypothesis *, intended to controvert the present received chemical doctrines. From the observation that the wire connected with the negative part of the battery gives out hydrogen gas, and the one connected with the positive side oxygen gas, he supposes water to be an element constituting the ponderable part of the oxygen and hydrogen gases, and the electricity to consist of two distinct principles, viz. positive and negative. The positive electricity, united to water, constitutes, according to him, oxygen gas; and the negative electricity forms, with the water, the hydrogen gas. As Richter's experiments have been found to be erroneous, the ingenious super-

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* This paper is not published. I was present at the Royal Society when it was read.

structure Dr. GIBBES has erected must necessarily fall to the ground.

It is, however, a curious circumstance, that water should be decomposed by two wires situated remotely from each other. To ascertain whether any particular current could be perceived, I placed a tube of water under the decomposing process, in my solar microscope, and applied to it a lens of about one eighth of an inch focus. The distance of the screen was six feet; and consequently the magnifying power was equal to more than three hundred thousand times*. This experiment forms one of the most beautiful microscopic sights I ever beheld. The two gold wires, being thus highly magnified, the disengaged particles of air appear like so many balls in rapid motion. I could not observe any difference in the sides of the wires, which might have induced a suspicion of the passage of a current from one of the wires to the other.

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* As the magnifying powers of the solar microscope are estimated according to the doctrine of similar triangles, the intersecting lines being in the centre of the lens, the area of any image on the screen will necessarily be in the inverse proportion of the distance of the object from the lens, compared with the distance of the object lens from the screen. The object being placed at the focal distance of the lens, viz. one eighth of an inch, and the screen on which the image falls being six feet from the lens, or 72 inches, we shall have $\frac{72}{1} \times \frac{1}{3} = 576$; and the surface exposed will be $576 \times 576 = 331776$.

In attempting an explanation I have indulged in some conjectures. I have already noticed, that oxygen gas, when condensed in an insulated ball, gives out greater signs of electricity than are evinced by the condensation of the other gases.

Oxygen gas is to be considered as a compound of oxygen base, of caloric and of electricity, while the base of hydrogen appears simply to require caloric to bestow on it a gaseous form.

Suppose A and B, *Plate* IX. *Fig*. 1, to represent two wires in a tube of water, A being the wire connected with the copper end of the battery, and B the one connected with the zinc extremity. Let P represent the particles of water round the wire B; and N the particles round the wire A. In whatever proportion the particles round B are influenced by the surcharge of electricity in the wire B, so, in the same proportion, the particles of water round the wire N will be influenced by the negative state of electricity. These changes diminish, in the duplicate ratio of the respective distance of the particles from the wires. Supposing C to be the central point between the above two wires, at this point a change will take place, because the particles of water at 3, 2, 1, have lost a portion of their natural electricity, by their contiguity to the negative wire A. The portion 3, which is the nearest, has been the first to lose; 2, the next; and so on. The particles of water round P have a surcharge, which at 6 is greater than at 5; and consecutively. The particles of water at C are the particles of communication, which are first subjected to the mutual influence of the two wires. When the constituent parts of water are disunited, and resolved into their oxygen and hydrogen bases, in order that they should assume a gaseous form, they must be united with other principles. Thus the base of oxygen, must acquire caloric and electricity; and until this has happened, it will not assume an elastic form. At the instant of disengagement, its capacity for electricity being increased, in obedience to the general laws of Nature it will move in the direction in which it meets with the least resistance, and which will be towards B. As soon as it is in contact with the wire, it will assume the gaseous state, in consequence of the addition of the electric principle. In the same way the hydrogen base, having its capacity for electricity diminished, will be similarly determined towards the wire A.

When pure ammonia * is placed in the glass tube instead of water, the decomposition is ex-

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* Dr. Moyes' observations.

tremely rapid, and the gases which are evolved consist of fifteen parts of hydrogen, thirteen of azotic gas, and nearly two of oxygen.

When, instead of a gold wire, a copper wire is employed, it becomes converted into a green oxyd, which must be the result of the action of an acid, the pure oxyds of copper being of a dark red or deep brown. This acid appears to be no other than the nitrous, formed by a combination of the oxygen and azote. When metallic solutions are placed in the glass tube, the decompositions are very beautiful; and the deoxydated metal is precipitated on the wire connected with the copper end of the battery.

CHAP. XXXIII.

*The comparative influences of electricity and galvanism on metallic substances—The metallic oxyds produced by electricity—The deflagration of wires and laminated metals, on their oxydation—Effects of deflagration on the gases—*BRUGNATELLI *on electrats—*VAN MARUM *on the effects of combustion on quicksilver.*

I HAVE been given to understand, that it was with the celebrated electrical apparatus at Haarlem, made by Mr. CUTHBERTSON, that the beautiful metallic oxyds were first struck upon paper, each of the metals having produced its peculiar characteristic tint. The mode of performing these curious experiments is represented in *Plate* IX. *Fig.* 2. A B is a plate electrical machine; C D a battery exposing a coated surface of about 18 feet; and R G a fine metallic wire, secured by pins in such a way as to be raised from the paper about one-eighth of an inch. Upon discharging the battery by the rod E, the wire explodes, and is dissipated in the air. Having been dispersed in every direction, a portion of it strikes on the paper, on which it impresses beautiful rays. A representation of these rays is given in *Plate* X., in which six of the metallic oxyds are tinted. *Fig.* 1, represents the appearance produced by gold wire; 2, the one produced by silver wire; 3, by copper wire; 4, by iron wire; 5, by tin wire; and 6, by leaden wire.

A powerful charge of electricity oxydates a considerable length of metallic wire; while galvanism acts on successive portions of the metal, whenever it is excited by a conducting medium. Electricity produces all its effects by one sudden and violent discharge; while galvanism operates by its continual current*. It is on this account, that the sensations produced by their diverse

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* The rapidity of this current cannot be ascertained. Valli employed conductors 200 feet, and Aldini 300 feet, in length, without being able to perceive any difference. When bodies move with an extreme rapidity, it is impossible to ascertain their velocity, unless a proportionate extent of space should enable an observation to be made, which is not the case with electricity or galvanism. The motion of light is determinable; and notwithstanding its velocity is immense, in passing through immense distances, the time it occupies in its transit is ascertainable, from such remote bodies as the satellites of Jupiter, and from the aberration of the fixed stars. Still, in so small a compass as the human body, physiologists have been vainly employed, in determining the supposed velocity of the nervous fluid, the existence of which is indeed hypothetical.

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actions, are materially different. The electric shock, from a very small jar, operates on the body by a sudden and percussive effect; while the one which follows the galvanic process seems to arise from a constant current, attended by a jarring and tremulous sensation.

All finely drawn metallic wires are rapidly burned by means of a large galvanic battery, the powers of which are in general ascertainable by the extent of fine steel wires, such as the pendulum springs of watches, which the battery will instantly render red-hot. This constitutes the most correct *galvanometer*; while the other, on which I have already touched, ought to be termed a *galvanascope*. A powerful battery, such as the one I generally employ, will induce a red heat on several inches of the above steel wire; and in this way I am enabled to ascertain whether my battery is in proper order.

To deflagrate finely laminated metals, good charcoal is the best medium which can be employed. As the substance of the metallic leaves, when laminated, is much thinner than that of any wires which can be drawn, the deflagrations are more brilliant, and the light extremely vivid. Some metals exhibit a different appearance from others.

Gold leaf, having the thickness of the $\frac{1}{282000}$ part of an inch only, is not merely deflagrated, but is likewise completely oxydated, and reduced to a purple powder.

Silver leaf, which is thicker than gold leaf in the proportion of 7 to 4, is likewise converted into an oxyd, and exhibits a beautiful greenish light. We thus see that, by the powers of galvanic electricity two metals which resist the utmost force of our most powerful furnaces, are rapidly oxydated.

Copper leaf, or, to speak more correctly, brass leaf, being the yellow Dutch metal made from copper plates, by cementation with calamine without a subsequent fusion, has a thickness nearly five times greater than that of gold leaf. In its deflagration, red ignited particles of copper are detached, on this account, that copper requiring a very high temperature before it fuses, the ignition takes place first. The copper, before it melts, is constantly red-hot, which is not the case with tin, lead, &c. The Dutch silver leaf, a composition of tin and zinc, about ten times as thick as gold leaf, is deflagrated with rapidity.

Tin-foil, which is also a composition of tin and zinc, is about 300 times thicker than gold leaf. Narrow slips of this substance are also easily deflagrated. In short, all metals, if in a due state of tenuity, are capable of being deflagrated.

When metals are placed under an exhausted receiver, they give out light, but are not oxydated.

When metallic leaves are deflagrated in carbonic acid gas, the light is not vivid; but in oxygen gas, the contact is no sooner established, than the metallic leaves are destroyed with one sudden flash.

BRUGNATELLI has conjectured, that the action of galvanism, in the decomposition of water, produces a compound which he apprehends to result from the union of the disengaged electricity with the oxygen, and which he terms an electrat. This electrat is capable of dissolving metallic substances. Thus, with copper, it forms a beautiful green; with zinc, a dark grey; and with iron, a reddish yellow oxyd. These effects, as I have already observed, appear to arise from the combination of nitrous acid and ammonia. They are probably produced in every galvanic decomposition of water*.

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* The atmospheric air, which is held in solution by the water, consisting of nitrogen and oxygen, when the water is decomposed, the oxygen, united with a part of the nitrogen, forms nitrous acid; and the hydrogen, in combination with a portion of the nitrogen, produces ammonia. By experiments in which I am at present engaged, I am persuaded that galvanism, on account of its surprizing subtility, and its permeable quality, will prove one of our most powerful chemical agents.

I expect,

Van Marum, in making the circuit through quicksilver, noticed a beautiful effect. The end of a fine wire having been made to form the communication, a powerful combustion ensued, which dispersed the mercury on all sides, and sparkles, having the appearance of thousands of rays, constituting fine suns of several inches in diameter, were produced.

I expect, by its means, to be soon enabled to ascertain, whether nitrogen unites more readily with oxygen, or with hydrogen. From the different facilities of combinations much may be deduced.

CHAP. XXXIV.

*On the galvanic process performed by particular animals—The sentiments of the ancient naturalists on the torpedo—The discovery of the gymnotus—*WALSH'S *experiments on the torpedo —*HUNTER'S *dissections —* Mr. NICHOLSON'S *torpedinal theory—The Voltaic principle applied to its explanation—Why torpedoes do not give out any light—*MUSCHENBROEK'S *observations on the gymnotus.*

THE surprizing property of an animal by which it appears to generate, and to determine through conducting media, at its will, a principle which occasions in the human body a benumbing sensation, was known to the earliest naturalists. HIPPOCRATES, PLATO, THEOPHRASTUS, PLINY and ÆLIAN, noticed this quality in a fish of the ray kind, which was on that account denominated torpedo. With the fall of the Roman empire, all sound learning was lost; but in the sixteenth century science again flourished. Then it was that BELON, RONDELET, SALVIANA and GESNER were eminently distinguished for their learning. The sentiments, however, of these naturalists, and the subsequent observations of REDI, BORELLI*, STENO, and REAUMUR were, as well as the inferences they drew, erroneous, when they treated of the property of this animal.

A few years after the discovery of the Leyden phial, a congre eel, or *gymnotus electricus*, possessing similar properties with the torpedo, was discovered at Surinam, and said by Mr. S'GRAVESANDE to produce the same effects as electricity. This discovery excited the philosophical ardour of Mr. WALSH, who instituted a series of experiments relatively to this species of animal electricity. The effects of the torpedo having been ascribed to the same principle, *gymnoti* and torpedoes were brought to England alive, at the private expence of this distinguished electrician; and the experiments which were made left no doubt as to their electrical properties.

This important discovery having been introduced,

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* The ancient naturalists attributed the shock of the torpedo to certain frigorific particles; and, from this reasoning, applied this fish, as a topical remedy, to inflamed surfaces. BORELLUS's idea is, that the shock is a convulsive sensation similar to the one which arises from a blow on the elbow. "Est torpedo piscis latus, similis Rajæ, molli tamen cute tectus: hæc, digitis compressa, tremore adeó vehementi concutitur, ut manum contrectantis molesto torpore dolorifico afficiat, ferè simili spasmo illo, qui producitur à cubiti concussione super mensam."—BORELL. *De Motu Animalium.*

duced, as a new principle, into natural philosophy, the extent of its application was regarded as infinite. It gave birth to several very bold conjectures, and there were few physiologists who were not persuaded that it promised a speedy developement of nervous influence, and muscular contraction.

Several of the above animals were dissected by the celebrated J. HUNTER, whose observations are published in the sixty-third volume of the Philosophical Transactions. The electric organ of the torpedo, as represented in *Plate* XII. consists of a number of columns, varying in their length from an inch and a half to a quarter of an inch, their diameters being about two-tenths of an inch. The columns in each of the organs of the torpedo, vary in their number from 400 to 1200, according to the size of the animal. These columns are composed of films, or membranous partitions, distant from each other about $\frac{1}{150}$ of an inch. From hence will readily be conceived the extent of surface which is exposed.

Mr. NICHOLSON has, in his valuable Journal, endeavoured to explain the action of the torpedinal system upon the principle of the electrophore. He conjectures the membranes to be non-conductors, and the fluid a conductor. At the time the above observations were made, they constituted the most ingenious and probable explanation which had been given. But since the Voltaic discovery of self-charging metallic plates, however surrounded by conducting fluids, the explanation of the animal electrical organs, upon the galvanic principle, may be readily given. VOLTA observes, that the interposed membranes answer no other purpose than the discs of moistened cloth between his metallic plates. I have already attempted to prove how electric charges may be produced in organic substances. On any action of the animal, the sides of the cells having been brought nearer to each other, a change as to their state of electricity results from their separation. It has already been remarked, that the changes induced on animal substances are small, when compared with those which are effected on metallic surfaces. As the sensation is in proportion to the series, and as, in each of the above electric animals, there are nearly 150 thousand cells, it must ensue, that the extreme smallness of the intensity of the fluid, will be compensated by the surprizing number of the cells.

However extraordinary this number may be, and although a surface of 4000 square inches is exposed, still the intensity is not able to remove the smallest interval of air, and does not therefore exhibit any light. On this account Mr. HENLEY denied that the phenomenon is electrical.

trical. Three hundred square inches of metallic surface will give a luminous spark; but this only takes place when the best conducting media are employed. Through water, or through animal substances, no luminous appearance can be produced by such an intensity; and the difference of the conducting powers in either case, has been already calculated by the learned CAVENDISH. In the larger electric animals, the *gymnoti*, luminous sparks have been produced. These animals seem to possess this property for the purpose of coming at their prey. As they are slow in motion, they can, by the means of such a principle, arrest the progress of other fishes at a certain distance. MUSCHENBROEK says that, having placed several fishes in the same vessel of water with a large gymnotus, they were all killed. He adds, that by the introduction of a lobster into the above vessel, the electrical animal was itself destroyed.

CHAP. XXXV.

The function of respiration considered as a galvanic operation—The lungs are, in structure, similar to the torpedinal system—Observations on Dr. Crawford's *theory of respiration—His experiments on the capacities of bodies for heat, not correct—New experiments on steam—Observations on heat—Dr.* Crawford's *estimates of the capacities for heat of arterial and venous blood—On the theory of the French Philosophers as to respiration—Calculations on the quantity of oxygen and nitrogen inspired—Cases in which there is no circulation through the lungs in torpid animals—Remarks on these particular lungs—Dr.* Wells' *remarks on the change of colour in the blood—How far electricity is concerned—This principle changes the colour of the blood—Remarks on the circulation—It is not general, as supposed—The colouring part of arterial blood never returns by the veins.*

THE various opinions of physiologists, relatively to the functions of the lungs, appear to have been finally concentrated in the doctrines of Dr. Crawford and of the French philosophers;

and these doctrines so nearly coincide with each other in their ultimate results, that it is now almost admitted as an axiom, that respiration is the source of animal heat.

I should not have presumed to controvert opinions, apparently so well established, if my arguments had rested on conjectures only; but as I am persuaded that my opposition to the above doctrine is supported by experimental facts, I feel myself divested of that diffidence I should otherwise have felt.

The introduction of remarks on respiration in a work on galvanism, may by some be deemed irrelevant; but every idea of incongruity will I trust be done away, when I set out by declaring my opinion, that respiration is an important galvanic operation, and that in their cellular structure, the lungs correspond to the torpedinal arrangement.

From the observations I am about to make, I have reason to presume that the agency of this principle will be evidenced in many important functions of the animal economy; and that many valuable deductions will follow, provided it should be made to appear, that this is the stimulating principle which Nature employs, to rouse into action the organized and animated parts.

Before any such doctrine can be admitted, it is previously requisite to examine those already laid down. If they should be found to be more simple, and more applicable to the explanation of the phenomena of respiration, than the one which I presume to advance, a demonstrative proof will be given of the erroneousness of my theory. But if, on the contrary, the generally received opinions should, on examination, be found insufficient, this consideration will, I hope, plead in excuse for my venturing to introduce a system of my own.

In the following inquiries, some deductions which I shall make, will perhaps, at first sight, be considered as bold and conjectural. I offer them however to the public, not as a *rudis indigestaque moles*, but as the result of long and mature reflection. They have been read before several of the London medical societies, where they found so little opposition, that I consider them to be stamped by the approbation of the learned members.

My review of the doctrine of Dr. CRAWFORD and of the French philosophers will not be minute, it being merely intended to shew that they are not adequate to the explanation of the phenomena of respiration.

Dr. CRAWFORD's theory is deduced from the different capacities of bodies for heat. By an extension of the experiments of FAHRENHEIT, BLACK and IRVINE, he was struck with the apparent difference between the capacities for heat in arterial blood and in venous blood. Conceiving this change of capacity to have taken place in the lungs, he conjectured respiration to be the source of animal heat. To demonstrate this, he instituted a variety of elaborate experiments, which appeared to him to confirm the supposition he had made.

By capacity, Dr. CRAWFORD wished to be understood, the power in a body to collect and retain heat. He accordingly proceeded to investigate the capacities of the principles which are concerned in the office of respiration, viz. oxygen gas, azotic gas, carbonic acid gas, vapour, arterial blood, and venous blood. In this order I shall cursorily examine his doctrine.

Dr. CRAWFORD's mode of ascertaining the capacity of any fluid, was by exposing a known quantity of it to a certain temperature, and then immersing it in a vessel of water, or oil, and observing how much heat it transmitted. Thus, having taken two tin vessels of an equal size and weight, each containing 37.91 cubic inches, one of them filled with atmospherical air, and the other with pure air, while the temperature of the room was at 58°. 1; these vessels, after having been heated in boiling water for ten minutes, were each of them plunged in vessels of the same size, each containing twenty ounces of water. This being added to the capacity of the vessel, was equal to 21.23 ounces of water. The vessel of atmospheric air heated the water to 59.5; while that of pure air heated it to 59.7. The air termed pure air consisted of two-thirds oxygen and one-third azote. The atmospheric air, of one-fourth oxygen, and three-fourths azote. As the difference of heat communicated was two-tenths of a degree, the Doctor algebraically deduced, that pure dephlogisticated air would yield .595, and the air of azotic gas .108. The water in the vessels was at 57.8 degrees.

To this deduction there appear to be several material objections. If it is to be admitted as a theorem, inferred from correct suppositions, we ought to find an exact proportion, an harmonious disposition, to take place uniformly in the subordinate members. This ought to be clearly demonstrated before it is admitted as an axiom, on which a superstructure is erected. In the first place, Dr. Crawford examined, by Dr. Priestley's method, the purity of the air employed. It is well known how materially experimenters have differed in their accounts of the quantity of nitrous gas necessary to the saturation of a given quantity of oxygen gas. Priestley observes, that one part of oxygen is condensed by two parts of nitrous gas; and Lavoisier by $1\frac{7}{8}$. Ingenhouz, Scherer and De

LA METHERIE state the quantity to be from three to five of nitrous gas; and HUMBOLDT, in the Annales de Chimie, asserts that the mean quantity is 2.55. Even in this particular, the least error in the estimate makes an enormous difference in the calculation.

It is rather singular, that the difference of temperature between the air in the room and the water, should have been so trifling; the air having been at 58.1 and the water at 57.8, or, in other words, there not having been more than three-tenths of a degree of difference. I have always found the temperature of the middle part of a vessel of water to be between the 25th or 30th part colder than the surrounding air, so long as the water is in a fluid state. According to Dr. CRAWFORD's statement, the vessel employed, ought, without its included air, to have raised the water 1°36 more than the heat communicated by the vessel containing the pure air.

It has been observed, that the homogeneous fluids absorb and transmit heat in the ratio of their densities. The quantity of atmospheric air employed in the above experiment weighed 11.78 grains; there was the same bulk of pure air, two-thirds of which, being oxygen gas, weighed 12.606 grains. There was consequently a surplus of one-thirteenth of the latter air, which must necessarily have transmitted more heat.

Dr. Crawford has also omitted to state the different degrees of expansion of the same air, as well as of the different gases at different temperatures.

The results of the different temperatures nearly correspond with the different weights of air employed.

In ascertaining its capacity with respect to water, Dr. Crawford seems to have been very incorrect. He first of all assumes Dr. Priestley's estimate of the specific gravities of pure air and atmospheric air, namely, as 187 : 185; and afterwards takes Fontana's estimate of common air and azote as 385 : 377. This is a proportion which cannot be correct*.

In his fourteenth experiment, Dr. Crawford acknowledges that, when atmospheric air was in each of the cylinders, the difference of the proportions of heat communicated was never greater than the one-twentieth of a degree.

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* Atmospheric air consists of $\frac{27}{100}$ of oxygen, and $\frac{73}{100}$ of nitrogen or azote. As the specific gravity of azote is less than that of oxygen, pure oxygen is to a mixture of $\frac{73}{100}$ of nitrogen and $\frac{27}{100}$ of oxygen, as 187 : 185. The difference between common air and nitrogen ought to be proportionably less, as there is less difference in the proportionate quantities of nitrogen: here the difference is made to be twice as much. In fact, the whole of the estimate is incorrect; the specific gravity of oxygen to common atmospheric air being 135 : 123; and that of atmospheric air to nitrogen, as 123 to 119.

In his calculation of the comparative heat of fixed air, or carbonic acid gas, he mentions very generally, that common air transmitted .034 more heat than fixed air. This is so loosely expressed, without any particulars of the experiment being given, as to preclude the possibility of any farther remark.

To his estimate of the comparative heat of inflammable air and vapour, there are many objections. The heat from inflammable air he found to be a little greater than from common air, with which it was accidentally mixed. Its specific gravity, compared to that of common air, will be, however, about 10 : 1 instead of 12 : 1. As this specific gravity is to water as 1 to 9960, he hence deduces that, in proportion, the quantities of matter in the inflammable air, and in the water, will be as 10567 : 1; and hence, compounding this ratio with the degree of heat communicated, that the comparative heat to water will be as 21.4 : 1*.

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* Inflammable air produced from diluted sulphuric acid and iron filings, has a specific gravity which is not more than $6\frac{1}{2}$ times lighter than atmospheric air, and always contains sulphuric

This air he supposes to communicate a little more heat than common air, to which he asserts that it would have been equal in point of heat, if it had been purely inflammable. This doctrine is strangely erroneous. If a certain quantity of inflammable air, when mixed with atmospheric air, has a greater capacity for heat than common air, surely if the whole mass were to be composed of inflammable air, the quantity of heat communicated would be greater still.

Dr. CRAWFORD endeavours to prove, by the experiments of GADOLIN and MAGELLAN, that the capacity of water for heat, is to that of snow as 10 : 9. As the quantity of heat absorbed, in the conversion of snow into water, is known, he hence calculates the point of entire privation of heat in water to be about 1400 degrees below the freezing point. Consequently, in inflammable air there ought to be disengaged 28,000 degrees of heat, when it unites with oxygen to form water: *i. e.* ten grains of inflammable air, when converted into a state of water, ought to evolve as much heat as would raise ten grains of water 28,000 degrees.

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ric acid gas. The inflammable air was preserved in a bladder. Now, it is well known, that such a fluid cannot be retained any length of time; insomuch, that a bladder replete with hydrogen gas, will, in a short time, be found to be filled with atmospherical air, the hydrogen gas escaping, and the common air passing in.

To ascertain this I made the following experiment:—Into a glass bolt-head I poured a mixture of muriatic acid and water * which had been some days prepared. The bolt-head was placed in a glass vessel filled with water: the quantity of water, the capacities of the vessels employed being added, was about equal to twenty ounces. I poured into the bolt-head a few steel filings; and having fitted, immediately after, a flexible tube to the pipe of the bolt-head, the hydrogen gas which was given out was made to pass into a glass receiver. When I had collected nearly sixty cubic inches, I found that the thermometer sunk down one degree and a half. This appears to me to be the exact difference of the degrees of heat existing in hydrogen when it forms a component of water, or is in the state of gas. Sixty cubic inches of hydrogen gas weigh 1.43784 grains, nearly one grain and a half; and this proportion raised twenty ounces of water one degree and a half, or an equal weight of water 10,000 degrees †.

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* Muriatic acid and water, being mixed with metallic substances, the hydrogen gas which is evolved is much purer than that which arises from the sulphuric acid, &c.

† For 20 ounces of water being equal to 9600 grains:

$$\frac{9600 \times 1\frac{1}{2}}{1.43784} \doteq 10{,}000 \text{ nearly.}$$

This does not correspond with the doctrine either of CRAWFORD or of LAVOISIER: upon what data the latter has ascertained the quantity of caloric in hydrogen gas, is not mentioned.

Having premised thus far, Dr. CRAWFORD proceeds to examine the capacity of vapour. He first attempts to ascertain the degree of heat given out by the combustion of hydrogen and oxygen gas, by means of the electrical pistol. This is incorrect, because an electrical pistol being formed of brass, gives out more heat by an explosion than a glass one would afford. I have found that my brass pistol, which only holds nine ounces, will heat a quantity of water, when exploded, greater than the proportion heated by my glass globe, which holds more than four times as much. He afterwards fixes the lowest degrees of heat, from these premises, at 1550, and takes Mr. WATTS's statement of 960° as the quantity of heat absorbed by water when converted into vapour.

As it was requisite that this point should be ascertained, and as Dr. CRAWFORD had taken his estimate from DE LUC's general statement, in which Mr. WATTS's mode of operating is not mentioned, the method cursorily suggested by BLACK, appeared to me to be the best, viz. that of ascertaining the quantity of heat imparted to a known quantity of water, by the method of distillation.

Into my refrigeratory were thrown 1540 ounces of water; into the still 280 ounces; and into a corresponding tub, placed at an equal distance from the furnace, nearly resembling the refrigeratory in shape, 1190 ounces of water. Three thermometers were employed; one in the refrigeratory, another in the corresponding tub, and the third was hung up in the room, the temperature of which was not quite 64°, at the same time that the temperature of the water was somewhat more than 61°. The temperature near the refrigeratory was observed on the distillation of every three ounces and a half of water. The condensation of this quantity of vapour raised the refrigeratory four degrees. At 91 degrees the evaporation took place; but did not sensibly cool the refrigeratory until the temperature was raised to 111°. It then operated so sensibly, that the condensation of the same quantity of steam raised the refrigeratory three degrees and a half. The evaporations increased in the subsequent condensations, and proportionately diminished the augmentations of temperature, until at length, the refrigeratory being at 167½, the evaporation was more than sufficient to balance the condensation. At half past twelve the connexion of the worm with the still was destroyed, and the gradation of cooling observed till seven in the evening. It was remarkable, that when it had cooled to 101, no sensible evaporation could be noticed.

By the distillation of 112 ounces the refrigeratory was increased in temperature $107\frac{1}{2}$ degrees; and, if there had been no evaporation, it would have been at 128 degrees. The heat from the furnace heated a corresponding tub of water seven degrees, and would consequently have heated the refrigeratory 5°.4. The capacity of the refrigeratory for heat was 120 ounces, making in the whole 1660 ounces of water. It raised 122.6 degrees, by the condensation of 112 ounces of water; and hence, by the simple rule of proportion, it will appear that one ounce of vapour contains as much heat as would raise one ounce of water 1817°.

The material difference between this statement and that of Mr. Watts, induces me to believe that he formed his vapour under a very diminished pressing state of the atmosphere; and in a partial degree of exhaustion. Mine was formed under an aerial pressure, corresponding to a mercurial column of 29.86 inches; rather more than the medium atmospherical state.

Mr. Watts[*] has observed that steam is to air, in specific gravity, as 4 to 9. Now, air being to water as 1 : 830, steam should be to water as 1 : 1867. It is for this reason that steam rises so rapidly in the air, every degree of heat nearly corresponding with its increase of velocity.

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[*] M. De Saussure a conclu, que les pesanteurs spécifiques des vapours et de l'air sont comme 10 à 14, au lieu que M. Watt

Matter does not seem to be regularly influenced in its expansion by equal portions of heat. Whether there be any general rule by which an estimate may be made, can only be determined by a great variety of experiments.

When matter, from being in a fluid state, becomes solid, or is converted into vapour, at the instant of its conversion into either state, caloric appears to be more freely disengaged. Thus WALLERIUS has observed, that the evaporation of ice is greatest at the moment of congelation. It has been very commonly observed, that the inferior part of a vessel of water is comparatively cool, at the instant even of boiling. This fact seems to confirm the doctrine of the ingenious Count RUMFORD, relatively to the conducting powers of heat, in different fluids, not being proportionate to their densities. Thus air, which is a worse conductor of heat than water, when rendered moist, is a more powerful conductor;

M. WATT les a trouvées comme 4 à 9 (ces't aprés lui que j'ai fixé cette pesanteur spécifique des vapeurs) *v.* DE LUC, § 8. p. 145.

when water is kept in a quiescent state, in a covered vessel, it may be cooled to 20°, without being frozen.

When matter, to undergo any changes, requires a great alteration of temperature, the expansions are more equal in smaller degrees of heat; thus mercury, which will bear three times a greater proportion of heat than water, before it either boils or volatilizes, will be more equably expanded. Thus, when the thermometer was employed, its correctness was evinced by the equal expansion of the mercury, in condensations of equal quantities of steam.

From the experiments of General ROY, it would appear that even in mercury there is a material variation in great differences of temperature. When the mercury was at 212°, the addition of one degree of heat did not expand it so much as when the temperature was at 112°. He found that the successive degrees of heat were not measured by equal increments of bulk in the mercury contained in the thermometer.

Common air he found to expand most, by one degree of heat, when at the temperature of 62°. Its expansibility was diminished in proportion as its density was decreased.

In water there appears to be no intermediate point between 212° and 1817; insomuch, that it seems instantaneously to acquire 1605 degrees of heat. Notwithstanding this, in heating water in a vessel, and marking the time when it reaches the boiling point, it might have been expected that, in equal portions of time, and in an exposure to an equal intensity, equal degrees of heat would have been imparted to the fluid. This is not, however, proved by experiment; seeing that a pint of water, which, when subjected to the action of fire, ought, by calculation, to have evaporated in thirty-eight minutes, requires more than double that time to evaporate.

The capacities of arterial and venous blood, into which an inquiry is made in the experiments of Dr. CRAWFORD, appear to me to depend on a different cause from the one he has suggested. The enthusiasm with which his doctrine inspired him, made him ardently apply the probable result of every experiment to its demonstration. In this he somewhat resembled that great genius JOHN HUNTER, who, from his physiological observations on the pigmentum of the eye, drew the conclusion that ADAM and EVE were not as MILTON has described them, but were born black.

With half a pound of water, at 53½ degrees, the Doctor mixed 9 oz. 4 drs. 14 grs. of venous blood, at 98° *. At the end of four minutes, the mixture was at 76 degrees.

Half

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* If we suppose, instead of venous blood, the same quantity

Half a pound of water at 53° was mixed with half a pound and 400 grains of arterial blood at 102°. At the end of four minutes the mixture was at 77½°.

If water of the same temperature had been simply combined with the water, the mixture would then have had a mean temperature. If, instead of venous blood, the same weight of water had been mixed, the temperature, allowing for the capacities of the vessels employed, would have been 77 degrees. But, admitting a loss of half a degree in pouring out the fluid, and of half a degree, at the same time, for the cooling, this will bring us precisely to the 76° observed in the case of venous blood. Hence, it would appear that the imaginary capacity of the latter is precisely the same as that of common water.

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tity of water at the temperature of the former to have been mixed with the half pound of water at 53½ degrees, and if we admit the capacity of the vessel to have been equal to half an ounce, as stated by Dr. CRAWFORD, then

$$\frac{8\frac{1}{2} \times 53\frac{1}{2} + \overset{\text{oz.}}{9\frac{1}{2}}\ \overset{\text{gr.}}{14} \times 98}{8\frac{1}{2} + \overset{\text{oz.}}{9\frac{1}{2}}\ \overset{\text{gr.}}{14}} = 77.$$

If, instead of arterial blood, water of the same temperature be substituted, as above, then

$$\frac{8\frac{1}{2} \times 53 + \overset{\text{oz.}}{8}\ \overset{\text{gr.}}{400} \times 102}{8\frac{1}{2} + \overset{\text{oz.}}{8}\ \overset{\text{gr.}}{400}} = 78.08.$$

If, instead of arterial blood, the same quantity of water, at the temperature of 102°, had been mixed, the mean temperature would have been 78.08. At the end of four minutes it was $77\frac{1}{2}$. By making our estimate as in the former case, the only result at which we shall come, will be, that the heat from the arterial blood is not so soon dissipated as from the venous. The reason of this may be explained on very simple principles: venous blood being more fluid than arterial blood, exposes, when poured out, a larger surface to the air, and will therefore be sooner cooled.

To imitate this process, I dissolved half an ounce of isinglass in a pint of water, and having raised the mixture to a temperature of 140°, blended with it a pint of water at 62°. Had the whole of the fluid been pure water, it would have been at 101°, but in this case it was reduced, by the capacity of the vessel, to 99°. When water was raised to the same temperature, and poured into a similar pewter vessel, precisely in the same manner, its temperature was somewhat greater than 98 degrees at the expiration of the first minute. It consequently sustained a loss, in the act of pouring out, more than half a degree greater than the loss sustained in the former case.

To found therefore a system on an observation of this kind, resembles the theories which were formed from the erroneous calculation made by BOYLE, of the specific gravities of serum and of the red globules. According to him, blood is to water as 1040:1000, but serum as 1190:1000. Instead of ascertaining the fact, the partisans of BOYLE fancied that an aerial spherule was attached to each of the red particles: this doctrine was received for nearly a century, until its fallacy was pointed out by Dr. JURIN.

Thus it is with the above system of Dr. CRAWFORD, the principal basis of which resides in its complexity; and its great security, in the apparent trouble of investigating its correctness. When we see a series of calculations, we are more apt to give credit to all the circumstances which are stated, than to undertake the arduous task of examining the principles on which they are founded.

The system I shall now proceed to examine, is that of the French philosophers, which, with the exception of some particulars it is not necessary to adduce, is as follows:

During respiration, or the passage of the blood from the right to the left side of the heart, a great quantity of oxygen is absorbed, by which the venous blood is rendered arterial. Carbonic acid gas and water are formed during the process; and hence the blood loses its hydrocarbonous impregnation, and has its capacity for caloric increased. The oxygenated blood passes on to the different parts of the system, through the medium of the circulation. During this passage, particularly in the capillary vessels, chemical changes take place. The oxygen uniting with the other elementary principles, caloric is evolved, and the phenomena of animal heat produced.

To this general statement there are several exceptions: some suppose all the phenomena to take place in the lungs. Others, among whom is Mons. SEGUIN, think, that the arterial blood becomes venous, in the extremities of the arteries, by absorbing hydrogen; and that, *vice versâ*, the venous blood becomes arterial by giving out its hydrogen in the lungs. On account of the affinity of carbonated hydrogen for oxygen, being greater than that of oxygen for caloric, and of carbonated hydrogen for the blood, water and carbonic acid gas are formed.

In the examination of this system, I have adopted the following method: First, to make some inquiries into the quantity of air taken in every inspiration. 2dly, To ascertain the changes this quantity undergoes; and whether, by these assigned changes, such a quantity of caloric is unfolded, as can maintain a mass of matter, similar to the one which constitutes the human body at 98°, whatever may be the state of atmospheric temperature.

The quantity of air taken in every inspiration has been differently estimated. BORELLUS *, in his eighty-first proposition, calculates it at about 14 cubic inches. JURIN †, HALES, HALLER, and SAUVAGE estimate it at nearly forty cubic inches. Dr. GOODWIN at about twelve; Dr. MENZIES at forty, and Dr. BEDDOES at twelve or fourteen. By frequent trials, I have found that I inspire on an average between sixteen and eighteen inches.

The precise quantity inspired is not of any particular importance, seeing that the estimate must be made from the quantity of oxygen gas which disappears.

It has been observed, that atmospheric air is the only gas which has the uniform power of supporting animal life. Even oxygen gas finally destroys life. As the latter stimulates animals too powerfully, so does common air act with an equal violence on fishes. Those with large gills are observed to die sooner than the others, and have the light red appearance of hyperoxygenation.

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* Hinc deduxi, quod moles aëris à me inspirati 14 digitos cubicos non æquabat; sed supponamus fuisse 15. BORELLUS.

† Æstimavi igitur aeris copiam, leni expiratione emissam tempore trium minutorum secundorum, numero rotundo, 40 digitorum cubicorum. JURIN.

Dr. DERHAM tells us, that in Holland they fatten carps by hanging them up in a net of wet moss in a damp cellar, with the head projecting from the net. In this state they feed them with white bread soaked in milk.

Dr. PRIESTLEY supposes, that a certain portion of nitrogen is absorbed by the venous blood. Dr. HIGGINS has drawn from his experiments the same conclusion. One hundred and sixty-one cubic inches of air having been respired for one minute, at the expiration of that time nine inches disappeared, and the residuum was found to consist of nitrogen, 111.6; oxygen, 23; and carbonic acid, 17.4. In this case, nearly twenty-four cubic inches of oxygen were absorbed, and somewhat more than seventeen cubic inches of carbonic acid produced. This was ascertained by the usual test of a solution of caustic potash*, and nitrous gas. The experiment was very accurately made by Mr. DAVY. I have repeated it in different respired quantities. The quantity of oxygen appeared to be on an average about 26, the thermometer being at 74°.

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* The saturating powers of nitrous gas were thus ascertained by Mr. DAVY. A pale green solution of nitrous acid in water, of the specific gravity of 1.301, is composed of 50.62 parts of water and 49.38 of acid: the acid is formed of 30 parts of nitrogen, and 70 of oxygen; hence the proportion is as 1 nitrogen, to 2.389 oxygen.

I repeated the experiment when the thermometer was at 55°, but did not observe any sensible difference. Mr. DAVY states, that the carbonic acid gas produced is nearly the same when respiring oxygen, common air, hydrogen, or the nitrous oxyd.

He respired 182 cubic inches of hydrogen. After six deep inspirations and expirations, the gas filled a space equal to 184, and consisted of carbonic acid gas 4.8; of oxygen 4.6; of nitrogen 21; and of hydrogen 153.6.

In respiring the nitrous oxyd, the same portion of carbonic acid is given out.

These experiments seem to prove that the carbonic acid gas, when thus evolved, is not the result of any chemical change during the state of respiration.

It would appear that it is given out by the venous blood.

On a supposition that the oxygen is absorbed, and evolves all its caloric, let us examine if this quantity will suffice for the purpose of animal heat. LAVOISIER tells us, that one pound of oxygen gas contains as much caloric as will melt 66.66 pounds of ice; now 26 cubic inches of oxygen gas would in this proportion melt 600 grains of ice. From this should be deducted the quantity of caloric abstracted from the system in the form of vapour.

Sanctorius found, that in Italy the perspiration was five-eighths of what had been taken in for food. Dr. Bryan Robinson observes, that a man on an average takes four pounds and a half of food during the twenty-four hours. That by perspiration are lost two pounds; by urine two pounds five ounces; and by stool three ounces. I have made the above estimate respecting myself, and find that I take nearly five pounds and a half of food daily, including liquids and solids. By the above proportion, the loss by perspiration will be nearly two pounds and a half, which is at the rate of twelve grains every minute. Dr. Hales found, that by respiration the vapour from the lungs was equal to two grains every minute: this, added to the other, will be fourteen grains per minute.

One grain of water at 172°* will melt one grain of ice; hence 14 grains of vapour should melt 148 grains of ice. This being deducted from the 600 grains, will leave 452 grains of ice, equal to 367 grains, or a little more than six drachms of water at the boiling point.

The quantity of circulating fluid in a human body,

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[*] Si par exemple, on prend une masse de glace à la température 32° de ce thermomètre, et qu'on la mêle à une masse égale d'eau à 172°; lorsque la glace sera fondue, on aura la masse totale en eau à 32°. De Luc.

body, is, at a medium, nearly 60 pounds. BARTHOLIN tells us of an individual who bled 48 pounds in three days by the nose; and in the Acta Erudita Leipsig, there is an account of a person who lost 75 pounds of blood in ten days. In the consideration of the temperature of the human body, both the solids and fluids should be taken into the calculation. These constitute a mass of matter equal to 160 pounds. Such is, at least, the estimate I have made of my own weight.

Into a large vessel I poured twenty gallons of water heated to 98°. The thermometer was at 66°. The temperature of the water was therefore 32 degrees higher than the surrounding temperature. In forty minutes it cooled to 90 degrees; or, in other words, it sustained a loss of eight degrees of heat. To restore it to its original temperature would have required 80 pounds of water at 212, at the rate of two pounds of boiling water every minute.

To ascertain whether this calculation was correct, the next day I had the temperature raised to 98°, and found that it required nearly three pints of boiling water every minute to preserve it at the same temperature*.

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* This shews that the decrements of temperature are not in the ratio of the times. During the first minute, the water cooled more than on an average of eight minutes.

I repeated this experiment when the thermometer was at 51°. It then required four pints and a half of boiling water to be thrown in every minute, to preserve the same temperature. What must it have required, when exposed to a great degree of cold? When PALLAS was in Siberia, a quarter of a pound of mercury became frozen by the natural cold. This was likewise experienced in Petersburgh in 1759, when the temperature was 74 degrees below the freezing point. GMELINUS, at Jeneseik, found it to be at 137 degrees below the freezing point. In 1760, an equal degree of cold was observed in Sweden.

The human body, so long as it retains life, preserves the same uniform temperature. Whether it is surrounded by a heated atmosphere, as in the experiment of Dr. FORDYCE, or is placed in the cold media above-mentioned, it will invariably be at 98 degrees.

We are told, that in winter the difference of temperature is compensated by a greater condensation of the air. It is generally observed, that air is expanded $\frac{1}{400}$ by the addition of every degree of heat. Now, between the temperature under the line, at 90 degrees, and that of the frigid zones above-mentioned, the difference of heat would be 190 degrees. Such a surprizing difference would not increase the density of the air one half, and would not be more than equal to one ounce of boiling water per minute; while, on the contrary, more than 100 times that quantity would be requisite to preserve such a mass as 160 pounds at the temperature of 98.

We thus see that, on every account, respiration cannot be the source of animal heat. Instances are recorded, in which the circulation through the lungs has been partially prevented, without the temperature having been changed. In the Observationes Anatomicæ of SANDEFORT, an instance is mentioned of a youth who died in his eleventh year. On the body being opened, the aorta was found to take its origin from both the ventricles. One and a half of the *valvulæ semilunares* were extended over the left ventricle, insomuch that only half the quantity of blood passed through the lungs.

Mr. ABERNETHY, in his excellent Essay on the Foramina Thebesii of the Heart, cavities which are well known to enable the nutrient vessels of the heart to relieve themselves when surcharged, remarks, that the foramen ovale is found open in all instances in which pulmonary consumption has for some time existed. This is owing to a deficiency of blood in the left ventricle, and to a redundancy in the right. The pressure of the latter on this membranous partition will stretch and irritate the uniting medium in such a way as to occasion its removal*.

That

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* At Rotherhithe, a boy ten years of age had the foramen ovale perfectly open. The surface of his body had so deep a venous tinge, that he was usually known by the name of the blue boy. There was an universal languor in his circulation. He was soon fatigued. The temperature of his body was 98°. This boy was subjected to several experiments by that excellent chemist, Dr. BABINGTON.

In the above case we find, that the same temperature was preserved, notwithstanding the circulation through the lungs was thus interrupted—a demonstration that the temperature does not depend upon any supposed decomposition taking place in the lungs; as the office of respiration is to afford a stimulating principle to the lungs, the want of air may account for the languor which was experienced in the above case.

In torpid animals, such as the hedge-hog, when in their state of torpidity, if the temperature of the surrounding air be even ten degrees below the freezing point, the animal heat has been always observed by that ingenious physiologist, Mr. ASTLEY COOPER, to be at 44°. At the instant they are awakened from their torpidity, the animal temperature immediately rises nearly 100°. This is a farther demonstration of the existence of a power inherent in the animal, to preserve a temperature above that of the surrounding medium, when the function of respiration is suspended. Such a temperature is adequate to the small share of activity which suffices for the corporeal preservation of torpid animals. It is no sooner roused, than the circulation recommences. All the secretory powers are in motion; and a higher temperature becomes necessary. This temperature is acquired much sooner than could be effected by the heat evolved in the first moment of respiration.

It

That so important a process as animal temperature should depend on the irregular, unequal action of respiration; that the functions of the animal economy should be exclusively governed by this uncertain, unstable process; would be to place the system in a very dangerous predica-

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It is well known that Dr. FORDYCE placed himself in a room, heated by the steam of water to 130°. The thermometer placed under his tongue stood at 100°. That the moisture with which every part of his body was covered, proceeded from the condensation of vapour, evidently appeared from a similar condensation on the surface of a Florentine flask, which had been filled with water, at 100°.

Mr. HUNTER has, by a series of very ingenious experiments, shewn the mode in which an animal body preserves its temperature in very cold media.

In the same way as many chemical compounds vary in their formation, according to the degrees of temperature, so it is with animal secretions. This may be illustrated by the nature of bile; the blood, in circulating through the vena portarum to the liver, or, according to one case which occurred to that excellent anatomist, Mr. ABERNETHY, through the hepatic artery, reaches those infinite ramifications which ultimately terminate in what has appeared to some to resemble cryptæ, to others, convolutions of the most minute vessels. Certain changes are there induced, by which the blood is converted into a substance called bile. It acquires a colouring principle, a bitter taste, and a mild mineral alkali, which the nicest chemical test cannot discover in the blood. This process is effected by the same powers (the powers of life), which provide for each of their processes the necessary degree of temperature.

ment. On every exertion, and on every sudden alteration of temperature, the secretions would vary; and the body would be in a state of constant changes. Is this consonant with the uniformity, the admirable regularity which is observed? with the inherent power, the *vis medicatrix naturæ*, which acts equally against excessive heat or intense cold? If these changes should be too violent for her efforts to withstand, her preserving temperature can only be destroyed by the destruction of life also.

In the Philosophical Transactions is to be found an ingenious essay, by the learned Dr. WELLS, on the changes the blood undergoes during respiration. The Doctor attempts to explain these changes by the principles of optics. He observes, with KEPLER, ZVCHIUS, and DELAVAL, that the colours of opaque bodies do not arise from the rays of light which they reflect from their anterior surface, but from the portion of light which, having penetrated their anterior surfaces, is reflected by the opaque particles diffused through the substance. The change of colour Dr. WELLS ascribes to the common air increasing the reflection of light from the internal parts of the blood. He found that the reddened pieces of crassamentum reflected more light than the black ones. We see many instances of bodies being influenced in their colours by the presence or absence of oxygen. Thus, Dr. BANCROFT observes, that the blue colour of a solution of indigo, depends on a certain portion of oxygen with which the solution is blended. When it loses this principle, it becomes pellucid. The Abbé NOLLET has remarked, that the tincture of archil, employed to colour the spirit of wine introduced into thermometers, after some time loses its colour, which it recovers when exposed to the air. It is the same with the infusion of turnsol and the syrup of violets.

With respect to the microscopic appearances, in a solar microscope of central rings, or opaque nuclei, enveloped in a transparent portion, described by Abbé TORRE, they are very visible. LEUWENHOEK imagined each particle to consist of six smaller globules, and these secondary ones of still smaller. With a very powerful solar microscope I endeavoured to examine the difference between arterial blood and venous blood; but could not observe any. These optical investigations do not tend to explain the office of respiration; but merely account for a secondary effect produced.

Doctors WHITE, GOODWIN, and many other physiologists, have deemed the office of respiration to be a stimulus to the circulation, and that, like all other stimuli, it gradually loses its effects. The pulse of a new-born infant beats

120 strokes in the minute; that of an adult 70; until at length the animal organization having lost its susceptibility of impression, dies for want of exciting powers.

If the circulation were to depend on the same cause as the evolution of heat, we should always find its increase contemporaneous. This is not, however, the case. Dr. CURRIE observes, in his valuable publication on fevers, that the vibrations of the arterial system were unusually slow, notwithstanding the animal heat was disengaged with more than four-fold rapidity. Even in the rarefied atmosphere of the summit of Mont Blanc, unconnected with any heat arising from the solar rays, the difference in sun or in shade was observed not to be more than two degrees, although SAUSSURE and four mountaineers experienced the most suffocating heat *.

BORELLUS †, conceiving that the arteries are so many inanimate tubes; that the whole motive power resides in the heart; and hence, *ceteris paribus*, estimating that its muscular actions are in the ratio of their masses, by comparing it with the temporal and masseter muscles, concluded that its action is equal to 180,000 pounds. KIEL, on the other hand, on an equally erroneous principle, reduced it to seven ounces. It was supposed by HALLER, that the velocity of the blood continually decreases; the sum of the transverse sections of the arteries greatly exceeding that of the aorta. It ought to be observed, that the thickness of the arterial tunics, relatively to their areas, is greater as the arteries diminish. When we feel the arteries of the head and feet, we find, as Dr. GILLUM* proved to the Chinese physician, that the pulsations are all consonant to each other.

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* See DE LUC Meteorolog. p. 367, v. ii.

† Vis motiva musculi cordis superat suo momento resistentiam totius sanguinis arteriarum et fasciarum, earundem dilatationem impedientium, quæ major est vi ponderis 180,000 librarum.—BOELLUS, *Prop.* lxxvii. *par secund.*

The principle which is imparted to the blood in the smaller arteries, is applied to a proportionably greater surface; and thus amply compensates for the loss which might have been supposed to arise from the increasing capacities.

The vesicular formation of the lungs, as it was discovered by MALPIGHI in 1660, exposes an immense surface. Dr. HALES† estimates that the vesicles are not more than $\frac{1}{100}$ part of an inch in size. If so, in the human lungs there will be about 30,000 square inches. Dr. KIEL calculates 21,906 □ inches.

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* See Sir GEORGE STAUNTON's Memoirs of the Embassy to China, v. iii.

† Dr. HALES found the capacity of the lungs of a calf to be 141 cubic inches. By abstracting the bronchia this will be equal to 100, for the cavity of the vesicles. The size of these vesicles

A proportionately extensive surface is observed in the structure of the gills in fishes, which are well known to correspond to the lungs in other animals. In a skate there are four doubled gills and one single one in each side, constituting eighteen sides or surfaces, on which the bronchial artery is spread out. On each of these sides there are about fifty divisions, or doublings of the membrane of the gills, each division having on each side 160 subdivisions, the length of which, in a large skate, is about one-eighth of an inch, and the breadth about one-sixteenth; insomuch that, in the whole of the gills, there are about 144,000 subdivisions, and the surface exposed is 2,250 square inches, or more than 15 square feet. When viewed with a microscope, after a very fine injection, every part is covered with a beautiful net-work of exceedingly minute vessels.

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vesicles he found to be about $\frac{1}{100}$ part of an inch in diameter. The sum of the surfaces, in a cubic inch, will be 600 square inches, multiplied by 100 = 60,000 square inches: deducting one-third for the absence of two sides in each little vesicular tube, the result will render it equal to 40,000 □ inches.

The air-bladder in fishes is not devoted to any purpose of respiration; but only calculated to increase, or diminish, the specific weight. This is not effected by any powers by which the air is condensed, as is generally supposed, but merely by determining the quantity of air from the portion of the bladder which lies in the thoracic portion of the fish, to the portion which is seated in the abdominal part. The bladder of a fish is formed of two sacculi, which communicate with each other. One of these sacculi lies in the abdomen, and the other in the thorax. The latter is surrounded by a firm muscular theca, or sheath, which, when it acts on the sacculus it envelopes, presses the air into the other sacculus. In this way, the abdomen becoming inflated, the fish rises.

These cells are similar in structure to the electric organs of the torpedo *, with this difference, that

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* In *Plate* XII. is a representation of the torpedo, with the electric organ on the side exposed. A and C are the two electrical organs, and B the integument taken off, in order to shew the cellular structure of the electric organ on the side A: the torpedo is a fish of the ray kind. This engraving is taken from the one preserved in the British Museum, by permission of my learned friend Dr. SHAWE.

These cells are, in their form and arrangement, very much like the air cells in the lungs. The torpedinal cells are never that the electric changes of the pulmonary cells are equalized by the pulsatory current of the blood; while the apparatus of the torpedo requires an external circuit. After air has been inspired, it has been observed by VOLTA and READ, that as soon as it is expired, its quantity of electricity is constantly found to be diminished.

As fluids are but imperfect conductors for the weak intensities produced by these processes, Nature has charged the venous blood with carbon, which being an excellent conductor, superior indeed to any other connected with the air-vessels, through this medium the electric charge passes, and, being instantly conveyed to the heart, stimulates that muscle to action.

Venous blood, when exposed to the galvanic process, soon acquires the redness of arterial blood. It has been deemed difficult to conceive how arterial blood should again become venous blood, and again acquire the carbonated tinge.

I must confess that I entertain great doubts as to any such change, and cannot persuade myself

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thrown into a state fitted for the galvanic operation, unless when they are in union with a body which is a conductor of electricity. In the lungs this principle is afforded by the change which the pure part of the atmospheric air undergoes. The residual air, when it is expired, is found to be in a negative state of electricity, with respect to atmospheric air.

self that the colouring part of arterial blood again returns into the veins.

The arteries contain blood, according to HALLER, in proportion to the veins as 4 to 9. According to other physiologists the difference is much greater.

The arteries terminate in four different ways, the most common of which is by branches, so very tenuous and minute as not to admit the red blood, and which are generally termed seriferous vessels. These vessels ultimately unite with the origin of the veins. The second mode of termination is into cells. The third is on the surface, where the exhalants are formed; and, fourthly and lastly, the arteries enter into the excretory ducts of the glands *.

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* From this review we see that red blood cannot pass from the arteries into the veins in a healthy subject. Even where the former of these vessels terminate in cells, no anatomical proofs can be brought of the return of the blood by the veins. When the portion of the blood which is devoted to the purposes of nutrition is already in its perfect state, it would be contrary to the simplicity Nature adopts, to have it again deteriorated, and again obliged to undergo a pulmonary change. In death the loss of power seems first to take place in the extreme branches of the arterial system, the remaining action of the larger arteries propelling the blood with a sufficient force to overcome the diminished resistance; on which account the arteries are usually found nearly emptied of blood.

When the nutritious part of our food is converted into chyle, and absorbed by the lacteals, these vessels all unite in one common duct, the thoracic, which conducts the fluid into the venous system. Even in the state of chyle the same globular particles are observable by the microscope as are found to exist in the blood. The particles there undergo a change in structure, and, becoming less opaque, instead of reflecting white light, exhibit a dark purplish blue light. When they have passed through the lungs, another change takes place, and they appear of a light Modena red. The red globules of blood have now undergone their last arrangement, and are then devoted to the restoration of the continual loss the system undergoes. By the colouring portion of blood, all new parts are formed. The arterial system never permits the red particles to enter again into the veins, unless in cases of disease.

The quantity of red particles received into the heart from the venæ cavæ may suffice for all the purposes of secretion. If, by any excitement, the capacity of the seriferous vessels should be increased, so as to admit the red particles, the latter will act upon the vessels not accustomed to receive them, as so many irritating extraneous substances, and induce inflammation. When blood is drawn freely from the arm, in separate cups, the last cup appears very different in redness

ness from the first, and possesses but little of the colouring principle; while no difference would be observed, if there were a free communication between the arterial and venous systems. As the terminating arterial vessels convey the colourless part only of the blood, in proportion as the veins empty themselves, the residual blood becomes more and more diluted with the serum from the arterial system.

Thus the circulation is in no way general; and the colourless part of the blood can alone be said to undergo a regular circulating course.

The principle which is unfolded in the heart acts also on the arterial system; and, diffusing itself throughout the whole frame, proves the source of general energy.

To the variation of this principle in the air, are to be ascribed the changes we undergo as to our sensations, together with the languor and indolence we experience, when the atmosphere is but feebly charged with electricity. So also, on the contrary, we may account for the cordial exhilaration we feel, when the barometer is high, and the air consequently charged with electricity.

This opens a boundless source of inquiry; and when we become acquainted with the stimulus Nature herself employs, much may be expected from its judicious application in cases where a want of action is required.

Nature, invariably simple in her operations, distributes to the particular parts of the animal economy, the blood in a state appropriated to the purposes she intends. Thus, oxygenated blood is not requisite to the secretion of the bile, notwithstanding it may be occasionally employed, as appears from a case of *lusus naturæ* which occurred to Mr. ABERNETHY, in which all the blood, returned by the veins of the different chylopoietic organs, was conveyed by the vena portarum immediately into the vena cava inferior, near to the origin of the emulgent veins. In this case, the hepatic artery*, which was somewhat enlarged, was the only vessel that conveyed the blood into the liver.

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* As, in other glands, the arterial blood answers the double purpose of affording a pabulum to the secretions, and of supplying the organ with vital energy, it has been supposed by some physiologists, that the hepatic artery does the same. They fancy that their conjecture receives some confirmation from the communication between the hepatic artery and vena portarum; but this is a communication which must necessarily arise from the hepatic veins returning blood from both.

CHAP. XXXVI.

The application of galvanism to medicine—Its effects in paralytic affections—In cases of deafness—The cases in which it is most beneficial—Whether it is useful in amaurosis, or in gutta serena—Observations in spasmodic affections—How far rigidity, and want of motion may be aided by this principle—Its good effects in indolent tumors—Cases of mental derangement considered—Its use in cases of suspended animation, whether from drowning or hanging.

HAVING attempted to prove the importance of the principle of galvanism, as it influences the animal economy, I shall now proceed to consider the particular cases of derangement in which the employment of this principle has been deemed beneficial.

I have already given an extensive account of the theory of Galvani, relatively to the existence of the two states of electricity in the muscular and nervous systems; and have also detailed his conjectures on the cause of violent rheumatic affections and of nervous sciatica. According to him, these diseases arise from extravasated humours,

mours, which form an arc between the electricity of the nerves and that of the muscles.

The influence of this principle is, I presume, merely to be attributed to its stimulating power. I do not think that, by any mechanical means, we can produce any increase or diminution of the principle of galvanism in any part of the animal frame.

I have already stated, that animal substances rank, in their conducting powers, next to metals; and that, in the gradation of these powers, they contain the principle of galvanism as one of their component parts. No change in these substances can therefore take place, without their capacities for electricity being changed also.

In the present state of our knowledge of the principle of galvanism, it has not been ascertained whether any particular corporeal derangements are the causes or effects of a change in the respective capacities of the diseased parts for electricity. The ascertainment of this point would be a considerable desideratum in medicine.

The action of galvanism on the muscular and nervous systems is more violent, in proportion to its intensity, than that of common electricity. The charge of a galvanic battery, so feeble as not to be able to disturb a gold-leaf electroscope, when propelled through the head, produces a stronger sensation than the one which is experienced from one hundred times the same intensity, produced by a Leyden phial.

In the case of the charged jar, such an intensity must be accumulated, as is able to act against the resistance of the air contiguous to the coatings of the jar. When it is discharged through any part of the animal frame, the same disturbance is not produced as by the passage of a galvanic shock.

The galvanism from a battery is discharged in that state of intensity only, in which it naturally exists in the animal frame. Every particle of the fluid being disturbed, the general influence is greater than if it had been acted upon by an intensity greatly superior to itself.

Having considered galvanism as the intermediate agent between matter and spirit, it may naturally be expected from this reasoning, that in the cases in which the influence of the mind on the animal functions is in any manner destroyed, as in paralytic affections, &c. the employment of the principle of galvanism should be attended with beneficial effects. Having already noticed the few cases which have hitherto been published, I shall now particularize those which have fallen under my own observation.

THE INFLUENCE OF GALVANISM IN PARALYTIC AFFECTIONS.

When paralytic affections arise from organic defect, as in cases of palsy of the lower extremities, from diseased spine, no good effects can be expected to result from the application of galvanism.

In cases of hemiplegia, arising from the pressure of extravasated fluid in or upon the brain, the stimulus of galvanism would, it appears to me, be injurious, in consequence of its action increasing the extravasation. In one case, of a person about the age of thirty, of a full plethoric temperament, who suffered the loss of the use of one of the sides, from an apoplectic attack, I applied gentle shocks of galvanism through the side of the brain opposite to the affected part of the body. Upon the third day, a considerable giddiness took place; and the vessels of the eyes became turgid. I immediately discontinued the application, and in a few days these unfavourable symptoms disappeared.

These cases are but few in number, when compared with those which arise from a defective energy in the brain itself. In these latter instances, galvanism has often proved of very considerable advantage.

By the means of galvanism I have never succeeded in perfectly restoring the patient to his original sensibility; but have, in many instances, considerably assisted the powers of motion.

The following is the mode I adopt in the application of this principle, supposing the right arm and right leg to be paralysed: My galvanic battery having been prepared as before directed, I begin with about a dozen plates, as in *Fig.* 2, *Plate* VII. If one of the wires be applied at B, and another at 2, the power is in proportion to the number of plates between B and 2. The nearer the other wire is placed towards C, the greater the power. In applying the conducting wire to the parts of the body through which the galvanic shock is intended to be passed, the continental practitioners have in general removed the resisting cuticle by the means of blisters. In having recourse to this application, which is unnecessary, the excoriated parts suffer greatly from the application of galvanism. The above mode was adopted, because, whenever the conducting wires are applied to the cuticle, the resistance through this non-conducting part is too great to be overcome by so feeble an intensity.

The method I employ renders this operation unnecessary. I simply moisten with water the parts through which I purpose to direct the galvanic fluid, and afterwards place on each of them a small piece, about the size of a shilling, of gold leaf or Dutch metal. On the conducting wires being applied to these substances, the circuit is completed.

In a case of hemiplegia, as before mentioned, a piece of gold leaf having been applied on the right side of the forehead, and another on the arm of the left side, as often as the circuit is completed, the arm becomes convulsively agitated. During the operation, one of the conductors should be left in contact with one of the pieces of metallic leaf, while the other conductor, which is employed to complete the circuit, should be removed immediately after the contact is made. The operator should proceed thus for about ten minutes or a quarter of an hour, according to the nature of the case, and the degree of inflammation induced on the parts.

Very soon after the application of galvanism, an areolous redness is perceived; and if it be persevered in too long, vesications and subsequent ulcerations are produced. These symptoms, which are a little troublesome for the moment, do not require any particular treatment in their cure. The part of the body to which the conducting wire from the copper side of the battery is applied, is always the most powerfully acted on; and if the conducting wires be kept in contact with the metallic leaves, for the space even of half a minute, without being removed, the one from the copper side will produce an acute pricking sensation, very much resembling the bite of a leech.

In some cases, attention ought to be paid to this difference between the ends of the battery. When it is wished to act on one part of the body more powerfully than on the other, the conducting wire from the copper end should be placed on that particular part. This difference in power corresponds with the effects experienced from the charged Leyden jar, in the case of which, the part of the body connected with the negative side of the jar is more powerfully acted on than the part connected with the positive side. In recent cases of hemiplegia, very good effects are soon perceived. After a few applications, a sensation of returning warmth is felt; and the action of the muscles of the arm restored, as often as the circuit is completed. After the operation, the use of the flesh-brush, persevered in for about a quarter of an hour, contributes to re-establish the circulation of the diseased parts.

The effects of this principle, when it is applied to the human body, are not so painful as those of electricity, notwithstanding its actions are more powerful. By preventing the muscles from remaining constantly in a state of torpid inactivity, the loss of substance which would otherwise take place is guarded against. The application should be made at least twice a day; otherwise the intervals will be so long as to prevent any good effects from ensuing.

In paralytic cases of the most distressing nature, considerable relief has been derived from the application of this principle. An individual, aged sixty-seven, residing in London-street, and well known to many artists as an eminent lay figure maker, had laboured under a paralytic attack nearly eighteen months before his surgeon sent him to me. All the usual remedies had, as well as electricity, been tried in vain; insomuch, that at the above period he was so totally deprived of motion, that he could not rise from his chair without assistance. He was brought to my house in a coach. For the space of about a quarter of an hour I transmitted the power of about twelve plates through the forehead and the back of the neck. This mode of application was continued for three or four days; and the fluid afterwards made to pass from the forehead through the affected arm. In less than a fortnight, he had recovered the faculty of motion to such a degree, as to be enabled to walk from his own house to mine, a distance of at least half a mile. In this state he remained in a manner stationary; and although the galvanism was still continued for a month or six weeks, still no perceptible change was observed to take place. Whenever he omitted the galvanism for two or three days, he found himself uncomfortable; and was constantly persuaded that, after its application, his mind was calmed, and not subject to so great a depression as is usual in paralytic patients. When it is deemed requisite to apply galvanism to any part of the body, in the case of which neither the head, nor the brain, enters into the circuit, the galvanic influence may be very considerably augmented. Thus, in a paralytic affection of the leg, which it is intended to galvanize from the knee to the foot, fifty or sixty plates may be employed, and will, in general, be found to be attended by good effects.

EFFECTS OF GALVANISM IN CASES OF DEAFNESS.

From the reports of the foreign practitioners, it would appear that galvanism is a specific in cases of deafness. Its success, in these cases, seems to depend on the cause which occasions the defect of hearing.

In that of persons born deaf, and necessarily dumb, it seems to arise from the diseased state of the semicircular canals, which, instead of being filled with a fluid capable of transmitting impressions, contain an inelastic, caseous-like substance. Under these circumstances, no good effect can be expected to be derived from galvanism.

When we consider the nice structure of the ear, it is not difficult to perceive by how many different causes deafness may be produced. Among these causes may be reckoned, obstructions in the meatus; a relaxation, or any morbid state, of the membrana tympani; a derangement of the ossicula, by the effect of which the tremors of the membrana tympani are not regularly transmitted, by the base of the stapes, to the membrane which covers the fenestra ovalis; any change in the watery property of the fluid contained in the canals; or any insensibility in the auditory nerve, by which its susceptibility of action is lessened. To these causes may be added another, which is, perhaps, the most prevalent, namely, an obstruction of the eustachian tube.

This tube answers, to the organ of hearing, the same purpose as the hole in its side does to a common drum. Without such an aperture in the latter, to admit the ready ingress and egress of the air, the vibrations of the parchment would be greatly impeded. When the head of the drum is struck, a pendulous like motion is produced in the parchment, which plays backwards and forwards. If the drum were to be air-tight, this motion would not take place, on account of the resistance of the confined air. This is precisely the case with the eustachian tube, which, opening into the posterior part of the mouth, prevents any aerial resistance to the vibrations of the membrana tympani.

It is fortunately in the particular species of deafness which can rarely be relieved by any other means, that galvanism is most advantageously employed, namely, in a defective energy of the auditory nerve.

This species of deafness is ascertainable, by the common practice of placing a sonorous body in contact with the teeth. If the communication of sound should not be thus rendered more distinct, we may conclude that the defect originates in the nerve; seeing that, provided the deafness had been owing to any derangement of the other parts of the ear, the sound, transmitted by the medium of the teeth, through the connecting bony substance, to the seat of hearing, would have been distinctly perceived.

In several cases, in which the deafness seems to have arisen from a relaxation of the membrana tympani, attended by a diminished secretion of cerumen, I have experienced good effects from the employment of galvanism, which not only induced a grateful warmth in the meatus, but also considerably augmented the secretion of wax.

In the instances in which the deafness, originating from an obliteration of the eustachian tube, has been of some duration, beneficial effects are not, in my opinion, to be expected to result from the application of galvanism. A mechanical obstruction is then formed, which might probably be increased by the continued employment of a powerful stimulant.

In the above cases of deafness, the public is highly indebted to the valuable observations of an eminent surgeon, Mr. A. COOPER, who has advised the making of a very small perforation through the upper part of the membrana tympani, by which means the ossicula will not be injured, and the air will be allowed to pass and repass through this artificial opening.

When it is ascertained that the deafness is of that particular nature, in which galvanism may be usefully employed, the following is the mode I have adopted for its application: In *Plate* XI. *Fig.* 1, is represented a trough, with two conducting wires, A and B, at the end of each of which is a small plate of ivory, about one inch and a half in diameter. Through the centre of each of these plates is passed a silver wire, with a small ball at its extremity. In order that they may be insulated, the silver wires, which are about an inch in length, are inclosed, each of them in an ivory tube. The inside of the right ear having been previously moistened with water, by the means of a pencil, the ball is introduced into the meatus, the ivory plate preventing, on any movement, the wire from penetrating too far. The other wire is introduced in a similar manner, into the left ear: when they are thus arranged, and preserved in this situation by the help of a ribbon, or of an assistant, the completion of the circuit is best effected, by bringing the end of one of the conducting wires into occasional contact with the trough. In the engraving, nearly all the plates of the trough are placed between the conducting wires A and B; but great care should be taken that a very gentle power be at first employed, not exceeding seven or eight plates. In this respect the quantity must be regulated by the sensations. Some persons scarcely feel the power of twenty plates; while others experience from such a proportion of the fluid a very distressing giddiness. It is always advisable to begin with the power of a small number of plates. If it be too considerable, a very painful sensation is induced; but if, on the contrary, it be so regulated as to produce sensible effects only, the sensation is then by no means unpleasant, and is followed by a very agreeable warmth.

I have sometimes found that, after the application has been made for a few days, a painful aching sensation has taken place on the side of the head. Having then discontinued the galvanism,

vanism, in a little time the pain has subsided, and the deafness been entirely subdued.

It frequently happens, that at the commencement the deafness appears to be increased. This has excited so much terror in the mind of the patient, as to have prevented him from submitting to a repetition of the treatment. I have, however, constantly found this increased symptom of ailment to disappear, and that, by a proper perseverance, considerable relief has in general been derived.

When a remedy is slow in its action, few persons possess the resolution to persist in the trial. They ought, on the other hand, to consider, that it is much safer that the change should be gradually effected, than produced by violent means.

When one ear only is affected, it is not necessary to introduce the apparatus into the meatus of the other. In these cases I have directed its passage through the affected ear, and to the hand on the other side. This is conveniently done by immersing the hand in a glass of water, in which the conducting wire is placed.

APPLICATION OF GALVANISM TO CASES OF WEAKNESS OF SIGHT, AND OF AMAUROSIS.

I wish it were in my power to speak as favourably of the effects of galvanism in complaints of the eyes, as in those of the ears; but as yet I have not met with one successful case. I have tried it both in incipient amaurosis, and in the completely formed gutta serena, without observing any favourable change whatever. In the application of this principle I have adopted every possible mode, either suggested to me by others, or conceived by myself, without having been enabled to reap any advantage. Although GRAPENGIESSER has published several surprizing instances of cures in the above complaints, still, from the variety of cases in which I have tried the galvanic influence for a period of several months, without having been so fortunate as to afford any relief, I now entertain but little hope of its efficacy in these cases. In chronic ulcerations of the eye-lids, I have been more successful, by producing a change in the morbid action of the part. In one case of blindness, in which I applied galvanism for a considerable length of time, the patient laboured under old ulcerations of the legs, which, from the influence of the galvanism on the system in general, took a favourable turn, and were in a short time completely healed.

INFLUENCE OF GALVANISM IN SPASMODIC AFFECTIONS, AND IN CASES WHERE THERE IS A DEFECT OF MOTION, OR A WANT OF ACTION.

In involuntary actions of the muscles, I know of no remedy so efficacious as galvanism. In a contracted state of the fingers, or hands, however violently the latter may be clenched, on the application of this principle for the space of a few minutes, it rarely fails to induce a relaxation. In cases of cramp, if of long continuance, and even of tetanus *, or locked jaw, it has afforded relief in a short space of time. In contractions of the joints, and in all cases of rigidity, it will be found a very advantageous stimulus, which will greatly contribute to the restoration of motion.

In the stiffness of the joints, occasioned by the gout, it has come under my observation that the stimulus of galvanism, conjointly with the flesh-brush, has been attended by the happiest effects. It seems to give such a tone to the blood-vessels as to render the circulation more vigorous. In this state of the disease such an effect is very desirable.

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* A very intelligent gentleman, General WATSON, informed me of a case of tetanus, which had resisted every other remedy, having been completely relieved by a quarter of an hour's application of this principle, to the convulsed muscles.

Perhaps in no case are the advantages of galvanism more sensibly experienced, than in indolent tumors, or scrophulous swellings, which have long remained stationary. By the influence of this principle, tumors of this kind have in a few days been brought either into a state of suppuration or resolution. Many swellings are of such a nature, that their removal by either of these means is desirable. I have frequently applied the galvanic principle with the utmost success in inguinal tumors, which had resisted every other curative intention. The obtuse aching sensation, generally attendant on these indolent tumors, is very speedily removed. In scrophulous affections of the neck, it has been found very beneficial.

The continental practitioners mention some cases of its utility in enlargements of the prostrate gland. In such cases I have never made trial of it, since I have always been averse to the introduction of any metallic substance into the urethra, it being constantly productive of great uneasiness. This circumstance has not escaped the penetration of Mr. Home, from whom I have learned, that he has observed the introduction of a silver catheter, to be invariably productive of more pain than a much larger sized catheter of elastic gum; and that he is persuaded of the existence of some irritating cause in metallic substances,

stances, which cannot be referred entirely to their durity. This appears to be the case with metallic bougies, which are always more distressing to the wearer than those manufactured from elastic gum *. Since the discovery of the universality of the principle of galvanism, there can be no doubt but that the introduction of a metallic substance, which is never free from an alloy, constitutes a simple galvanic combination. On this principle its irritating cause is easily explained.

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* The great advantages which are derived from having these valuable instruments manufactured of materials possessing all the requisite properties of pliability and smoothness, and incapable, at the same time, of being acted on by the animal fluids, have occasioned considerable attention to be bestowed on the chemical arrangement of elastic gum, the substance the best calculated for such a purpose, when it is properly prepared. The bougies thus manufactured undergo no change, either by use or by a diversity of climate. About a dozen, consisting of all the different sizes, will consequently suffice any person for life. They are admirably calculated for the East or West Indies, where strictures in the canal are very prevalent. To this process I have devoted many years attention; and have now ultimately succeeded in bestowing on the elastic gum so even and smooth a surface, as to make it constitute the easiest bougie now in use. These bougies may be had in proper sets at No. 19, Soho-square, London.

INFLUENCE OF GALVANISM IN CASES OF MENTAL DERANGEMENT.

So few are the means we possess to relieve those who unfortunately labour under intellectual derangement, that whatever presents the faintest prospect of success, claims the attention of the practitioner. In the records of the medical application of galvanism, two remarkable instances of its good effects, in maniacal cases, have been adduced by ALDINI. One of them afforded an instance of a gradual diminution of the energies of the mind, which ultimately sunk into stupidity; and in the other case, which was of a directly opposite nature, the system was in a state of violent excitement, and the patient raving and unmanageable.

Melancholy madness is accompanied by an universal inactivity; a torpor in the vascular system; a paleness of the countenance; a coldness of the extremities; a contraction and shrinking of the skin, over the whole of the surface of the body; a smallness and slowness of the pulse; a want of appetite; a deficiency of muscular force; and a sensation of languor which overspreads the whole of the frame.

These symptoms are the effects of the impression on the mind, whether it be occasioned by grief, sorrow, or fear, and fully demonstrate the reciprocal action and re-action which exist between the corporeal and vital parts. My very ingenious friend Mr. HASLAM, in his observations on insanity, has, with a certain share of humour, ridiculed the idea of a disease of the mind. The great opportunities he has had to examine all the varieties of mental derangement, and the correct descriptions he has given of them, persuade me, notwithstanding, that he must be convinced of the actions of the vital principle being deranged in these diseases.

The opinions of the above physiologist border on materialism. He supposes, with PRIESTLEY, that matter may be so arranged and organized as to be able to think. This persuasion, according to him, derives some support from the diseased appearances of the brain; and to organic affections of this nature he ascribes the incorrect association of ideas.

If such were in reality the case, the same state of derangement ought to accompany similar appearances of the brain; but the cases which Mr. HASLAM has adduced prove the contrary.

What the principle of life is, our limited faculties do not allow us to ascertain. All we know is, that there is an active something which does not possess the properties of matter; and that all its laws and actions are peculiar to itself. To suppose that matter should, by any state of exility, be capable of simple sensation, would be as difficult as to conceive that mites are the result of a fortuitous arrangement of caseous particles; or that the elephant is the chance offspring of the wood he inhabits.

In cases of mental derangement originating from the passions of grief, sorrow, or religious fear, and in which the system has sunk into apathy and dulness, the stimulus of galvanism affords some prospect of success, more especially if the patient be not advanced in years. It is in general supposed, that deranged persons require the action of more powerful stimuli on their respective organs, than persons in a state of sanity. Mr. HASLAM has however proved, that a difference is not requisite in the doses of medicines administered to them; although it has been usually remarked, that they suffer less from operations performed upon them than other individuals.

In one of the cases ALDINI has described, he employed a pile consisting of eighty pairs of silver and zinc plates. Its application was directed through the upper part of the head, one of the hands being placed in a glass of salt and water. At the end of two or three days, the patient smiled, as if to denote that the sensation was pleasurable. After a few days had elapsed the head was shaved above the frontal sutures, and moistened with salt and water. The galvanism having been directed for several days through this part, in a little time the patient recovered.

From the effects I have noticed, in the application of galvanism to the brain, I should not be induced to employ, at the commencement, such a series of plates as the above. It is better to be slow and gradual in augmenting their number, than to subject the brain to too violent an action.

In the species of delirium which is termed hypochondriasis, in which a number of symptoms, evincing a deranged state of bodily health, occur, before any alienation of reason takes place, the stimulus of galvanism promises considerable success. The symptoms indicate a disordered state of the stomach and intestines; flatulency; a sensation of suffocation; and an acidity, connected in general with a costive habit. In this case gentle shocks of galvanism, sent through the stomach and diaphragm, may tend to correct these morbid actions *.

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* A case of hydrophobia, which consists of derangement of the intellectual powers, occasioned by the application of poison, was completely cured by powerful shocks of galvanism sent through the head. The mind is surprizingly influenced by this principle, when directed through the brain. The sensation is peculiar, and cannot be imitated by electricity.

In the distressing complaints to which females are so very subject, and which are usually termed nervous headachs, attended by a violent oppressive sensation over the eyes, together with nausea, and an almost entire inability of motion, I have derived the greatest advantage from the employment of galvanism, by directing the power of about a dozen plates through the temples.

In a case of ideotical derangement, of nearly ten years standing, originating from a suppressed mercurial action, I tried the effect of powerful shocks through the brain, for some weeks, without observing any particular advantage.

EFFECTS OF GALVANISM IN CASES OF SUSPENDED ANIMATION.

When the extraordinary influence of the principle of galvanism, on the muscular fibres of dead animals, was first observed, it was natural to expect that great advantages would result from its employment in those particular cases in which life is not extinguished, but its influence on the animal organization merely suspended.

The divided part of an animal, when cut off from the sources which might be deemed absolutely requisite to the support of its living energies, still evinces manifest signs of the existence of a vital principle, when roused into action by galvanism *.

The muscular contractions induced by the influence of a small portion of electricity, were strongly manifested in the experiment which led GALVANI to his important discovery. This is represented in *Plate* VIII. in which A B is a plate electrical machine; C the prime conductor; D a brass ball held at a distance of two or three inches from the prime conductor by an assistant; W the experimenter, with a prepared frog, E, on a plate, and touching the muscles with a dissecting knife. As often as a spark passes from C to D, the muscles of the frog are thrown into action, even at a distance of two or three feet from the machine, with which it is perfectly disconnected.

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* In the plate at the commencement of the first volume, is represented the head of an ox thrown into convulsive motions by the influence of six galvanic batteries. When the head is warm, and has not been long separated from the body of the animal, the convulsive actions are very considerable. The eyes open spontaneously, and the pupils become dilated. The ears and horns move with a considerable force; and when the tongue is drawn out, and even secured to the table, by perforating it with an iron skewer, which is made to penetrate into the wood more than half an inch, I have frequently observed a retraction of that organ, so powerful as to detach the skewer from the table, and to throw it up into the air, to the distance of nearly a yard. When I subjected an entire sheep to the galvanic influence, the motions resembled the convulsive struggles of animals in an epileptic state, and were much more powerful than the natural actions. The horses recently killed, which I likewise galvanized, required two persons to restrain the motions of each of the legs.

In the living animal, when the muscles are under the controul of the will, the contractions are very feeble, when compared with those which are produced on this connexion being destroyed.

ALDINI erroneously supposed that the heart, in common with all the involuntary muscles, is not susceptible of the galvanic influence. NYSTEN has, on the contrary, asserted that the heart retains this faculty longer than any other muscle. The observations of the latter of these naturalists carry with them such an air of suspicion, relatively to the accuracy of his experiments, as to merit very little attention. VASSALLI-EANDI remarks, that when the heart is acted upon within thirty or forty minutes after death, the contractions are evidently perceived. HUMBOLDT and GRAPENGIESSER have related instances of the intestines being acted upon by galvanism; and my friend Mr. CARPUE has assured me that, conjointly with Dr. PEARSON, he has produced similar effects upon the alimentary canal.

When life is suspended*, and the principle of irritability not destroyed, the stimulus of galvanism, if prudently employed, may rouse the dormant energies of vitality, and restore the system to its naturally active state. Fortunately, in most cases of asphyxia, the principle of irritability is not in any degree lessened. An animal, whether drowned in water or in hydrogen gas, exhibits nearly the same symptoms. When exposed to either of these media, the pulse soon becomes weak and frequent; the animal feels at the breast an anxiety which it struggles to relieve; and these difficulties increase until it falls down without sense or motion.

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[*] My learned friend, Professor COLEMAN, in the introduction to his Dissertation on Suspended Animation, deems the expression, *suspension of life,* as not physiologically correct, and has substituted the term *suspended respiration.* It appears to me, that there are greater objections to this latter term than to the former. In that particular state of the system in which all the animal functions are quiescent, not only the respiration, but also the circulation and all the secretions are inactive. The change induced is better expressed by the general term of *suspended animation,* than by particularizing the suspension of one organ only. The term *suspension* cannot imply either abolition, or total destruction; and hence, in cases in which the powers of life are not destroyed, but merely dormant, the word *suspension* very clearly expresses that particular state of the animal functions.

Dr. PRIESTLEY observes, that hydrogen gas kills animals as soon as carbonic acid gas; but this has been found not to be the case. SCHEELE was enabled to make 20 inspirations of hydrogen gas without much inconvenience; and I have myself frequently made nine or ten. Dr. BEDDOES states, that a rabbit which had been immersed in hydrogen gas for the space of seven minutes, was afterwards recovered. In hydrocarbonate gas, one of these animals was quite dead in one minute and a half; in carbonic acid gas another was perfectly irrecoverable after one minute and a quarter. The noxious airs to which we are principally exposed, are the carbonic acid gas, constituting the choke-damp, and hydrogen gas, which, in mines, is usually termed wild-fire. The suspension of vitality, by immersion in either of these gases, or in water, in no degree diminishes the irritability of the muscular fibre. It is not, however, the mere excitement of the muscular system, with a view to the renewal of its action, that will suffice for restoration; its employment must be combined with other means.

STRUVE, in his Practical Essay, observes that electricity ought not, in cases of suspended animation, to be resorted to without a mature consideration; but his directions are not founded on good philosophical principles. Professor COLEMAN ascribes the suspension of life to a collapse in the lungs, by which, he says, a mechanical obstruction in the interior pulmonary vessels takes place, with a want of latent fire in the blood. This Crawfordian doctrine of the evolutions of heat, I have already examined. Upon this idea the ingenious Professor observes, that the indiscriminate electrification of every part of the body is more injurious than beneficial; and that more especially, by an attempt to stimulate the heart, while a collapse of the air-cells exists as a cause which must impede its action, its irritability would be destroyed, and no efficient action produced. It is well known, that in proportion as the muscular fibres are in action, so the irritability diminishes. Thus, in a prepared frog, exposed to a powerful galvanic apparatus, the muscles soon become rigid, and incapable of action. Upon this very rational principle, the Professor recommends the previous expansion of the lungs. The heart being now acted on, beneficial effects may be expected to result.

In cases of suspended animation, whether from drowning, hanging, or exposure to noxious gases, the body should be divested of its cloathing, and placed in a warm bed, nearly approaching to the natural temperature. If it can be procured, air with an increased proportion of oxygen should be introduced into the lungs; and, at the same instant, very gentle galvanic shocks should be sent through the body, in such a direction as to influence the heart. By combining this principle with the other usual means, the most advantageous effects may be expected. In the case of Forster, five or six hours after his execution, such actions were produced by the galvanic apparatus, as to lead the practitioners who were present, to suppose that, at this late period even, a recovery might, by perseverance, have ensued. The general idea that, in cases of hanging, the vertebræ are dislocated, is erroneous. Very few instances have occurred of any organic derangement; insomuch, that whether the death be occasioned by hanging or drowning, the cause is the same.

I have constantly entertained a persuasion, that complete death takes place from the arteries being emptied of their contents. In the last convulsive agonies, those arterial terminations, which do not admit the passage of red blood, having lost their resisting power, suffer the whole of the blood to be emptied into the venous system. In this way the energy of every part of the body is destroyed.

Having thus considered the principle of galvanism in its operations upon animate and inanimate matter, I presume that this subject will be deemed worthy of our most serious attention. It enables us to refer to the same cause many beautiful phenomena in Nature; and is an additional argument in proof of the simplicity by which all her processes are effected. The discovery of this principle has contributed to remove, in a certain degree, the veil which has hitherto prevented us from comprehending a variety of changes in the material world.

On a supposition that galvanism is the intermediate principle between matter and spirit, I cannot, I must confess, conceive the mode in which the agency is effected. To comprehend the essence of our own animation, requires the powers of a principle superior to that which we possess. Infinite as I regard the difference between common matter and our vital principle, still we may suppose another infinitude, between our spring of life and that source which comprehends all.

Quippe haud tam locuples hæc, tamque immensa, suppellex
Corporis in cellis poterit stipata teneri,
Aut vi corporeâ revocari in luminis oras,
Quænam isthæc: nisi vis divinior, ætherisque
Sensus, et afflatu cœlesti concita virtus? *

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* ISAAC H. BROWNE, de Animi Immortalitate.

APPENDIX.

SINCE Chapter XXXI. relative to the limitation of charges, as observed in a series of very small plates, has been printed off, I have made various experiments on batteries consisting of plates of different sizes, and have observed that, with those of the larger size, there are certain limits; insomuch that, in continuing the deflagrating operation, numbers are to be consulted.

In a battery consisting of fifty plates, each measuring eight inches on one of its sides, and in which, consequently, the area of the whole of the zinc surface exposed is equal to 3200 square inches, the first effect upon a piece of fine wire is in the ratio of the surface. The second effect, if quickly repeated, is considerably weaker; and its powers diminish rapidly. If the battery be left for about ten minutes, it will then accumulate nearly the same powers.

If two metallic conductors are brought into contact for a moment, a fine wire applied immediately afterwards will not be deflagrated. Gold-leaf itself will be but faintly scintillated.

If such a surface be employed, by the means of four-inch plates, it will require two hundred of them, to be equal to the battery above described. Although the surfaces are equal, the deflagrating effects are more uniformly preserved in the latter battery.

From this remarkable circumstance it would appear, that the galvanic operation is not proportionally active in large plates as in small ones. We may hence deduce, that it is not necessary, for any experiment whatever, to employ larger plates than those of four inches; and that it is expedient, according to the effects we wish, to consult numbers. For continual brilliant deflagrations, 300 four-inch plates will suffice.

In using the batteries, great care should be taken that the same kind of mixture, in charging the battery, be employed. If diluted sulphuric acid be once used, the effect of muriatic acid afterwards will be considerably less than if the latter acid had been first employed.

INDEX.

A

VOL. PAGE

- AIR, atmospherical: its influence on galvanism; and functions, as a medium, - i. 224, 225
  - principle of its diffusion in water, - ii. 224
  - the idea of a vacuum incompatible with the explanation of the apparent exhaustion of a pneumatic receiver, - - ii. 225
  - influence of electricity on air, - ii. 233-236
  - proofs of the action and re-action of air and electricity, - - ii. 238-242, 273
    - theory of the Leyden phial, - ii. 259-265
  - remarks by Dr. CRAWFORD on the proportionable capacities of different kinds of air to retain heat, (See *Heat*), - - ii. 404-409
  - at what temperature common air expands most by heat, - - - ii. 415
  - calculations of the quantity of air inhaled by respiration, - - ii. 421-423
- ALDINI, Professor, nephew to GALVANI; his experiments and observations, - i. 387
  - contractions excited without the intervention of metallic substances, - - i. 387
    - by the application of silver alone, - i. 388
    - inferences, - - - i. 388, 389
  - experiments in the presence of two commissioners of the French National Institute, - i. 389
  - his endeavours to reconcile the theory of VOLTA to that of GALVANI, - - i. 390, 391
  - his medical application of galvanism, - i. 393
- *Amalgams*; best preparation of, for the excitation of electricity in glass, - (note) ii. 231
- *Amaurosis*, application of galvanism to the cure of, i. 465, ii. 455
- *Ammonia*, experiments respecting the acid solutions of metals with, - - ii. 60, 61
- *Amphibious animals*, effects of galvanism on, i. 291-293
- *Amputated limbs*, experiments on, - i. 71, ii. 191

Animal

INDEX.

VOL. PAGE

Animal heat, analysis of M. Josse's inquiry respecting, - - i. 172

system relative to caloric, - i. 172

application of these principles to animality, i. 173

influence of different temperatures on animated beings, - - i. 174

Animal substances, galvanic combinations produced by, - - - ii. 333-342

Animals, description of those best calculated for galvanic experiments, - - i. 9

various phenomena in the animal system explained by galvanism, - - - i. 21,22

state of electricity in the animal during galvanic operations, ascertained, - - i. 34

experiments respecting the influence of the galvanic fluid on those parts of animals not governed by the will, - i. 207-212, 266, 302-305, ii. 197

universality of the influence of galvanism on animals, - - i. 218

conditions necessary to produce in animals the galvanic phenomena, - - i. 226

influence of galvanism upon the different classes of animals: amphibious animals; birds; fishes; insects: see those articles: Upon man, i. 293

method of exciting the galvanic phenomena in animals, - - i. 363,364

experiments on the means of varying, enfeebling, and re-establishing, the susceptibility of animals, i. 376,377

See also the articles *Blood*; *Frogs*; *Muscles*; *Nerves*; *Time*; and *Vitality*.

Aphony, arising from a defect of the nervous action; application of galvanism in the cure of, i. 428,434

Apoplexies; phenomena of, compared by Galvani with the effects of electricity, - i. 401

Arcs: the two species of, the *animal* and the *exciting*, explained, - - i. 364

parts of the animals which are essential in the arc, i. 241

experiments and conclusions by Halle respecting the essential parts of the *animal* arc, and their dispositions, - - i. 365,366

respecting the parts of the *exciting* arc, and their relative nature and dispositions, - i. 368-372, 375

definition of the exciting arc, - (*note*) i. 188

general remarks respecting exciting arcs, i. 58, 72, 73, 250

peculiarity of the leaden exciting arc, - i. 32

— effect

VOL. PAGE
— effect of the metallic arc in an insulated state, i. 33
different results of different methods in the application of the exciting arc, - - i. 40
phenomena in the operation of various metals as exciting arcs, - - i. 45
conditions essential in the arc, to produce in animals the galvanic phenomena, - i. 239
inquiry whether the employment of the arc always necessary, - - i. 239,240
constituent parts of the arc, - i. 241
classification of the different substances, i. 242,243
question of those which are inaccessible to the galvanic fluid, - - i. 244
respecting the cause of the different degrees of susceptibility, - - i. 245
modifications of the galvanic forces, produced by chemical mixtures, - i. 245
by the friction of certain bodies, - i. 246
by the form of the exciting arc, - i. 248
by the diverse arrangements of the arc, i. 248
effect produced by the employment of two arcs in the same part, - - i. 252
inquiry respecting the manner in which the galvanic phenomena are modified by the application of the arc to the animal, - - i. 257
Army and *Navy surgeons* should be provided with a simple galvanic apparatus in inspecting the bodies after an engagement, - i. 415,416
Arteries, experiments on, - i. 154-157, 210-212
capacity and structure of, - ii. 437
Asphyxy; experiments relating to the effects of, on the muscular organs, - - i. 378
reflections, - - i. 379,380
knowledge of the nature and treatment of asphyxies to be derived from galvanism, - i 385
employment of galvanism to distinguish real death from asphyxy, - - i. 406
objections relative to the efficacy of this method, i. 407
observations of HUMBOLDT upon, - i. 410
his experiments on this subject, - i. 412
remarks, - - - i. 413
uncertainty of this criterion, - i. 414
not to be rejected entirely, - i. 415
question whether it is capable of recalling to life individuals apparently dead, - i. 416

VOL. PAGE

—— experiments on animals, i. 49, 50, 51
medical application of galvanism to asphyxies, i. 429, ii. 466
Atmosphere, experiment relative to its influence on the phenomena of galvanism, ii. 81
varying state of, dependent on the principle of electricity, ii. 274
production of various atmospherical meteors, ascribable to the same cause, ii. 276
method of determining the height of the atmosphere, *(note)* ii. 277
Aurora borealis, production of this phenomenon dependent on electricity, ii. 278, 279
when first observed, *(note)* ii. 279
observations respecting its nature and appearance, ii. 280, 281

B

Battery, galvanic; definition of, ii. 300
its construction, and operation, ii. 351
improved by the addition of a wooden trough, ii. 353
advantages of this appendage, ii. 355
fluids most proper to be employed, ii. 355
choice of metals for plates, *(note)* ii. 358
effect of the battery proportioned to the *number* of plates, and not to the extent of their surface, ii. 359, 364
experiment, ii. 365
theory of this circumstance, ii. 360, 365, 366
inequality of the cells, ii. 367, 368
the cells should be insulated, ii. 371
method of connecting several batteries when necessary, ii. 357
in combining several batteries, they should be of equal force, ii. 372
shocks produced by batteries of different magnitudes, ii. 18, 19
powers of water, and of the human body, as conductors, ii. 362-364
employment of the battery in the practice of medicine: (See *Medicine)*, i. 424
modes of its application different in different diseases, i. 430-434
decomposition of various fluids, effected by the battery, ii. 374

VOL. PAGE

— deflagration of metals: (See *Deflagration*), ii. 390
experiments with the Teylerian battery, ii. 107
See also *Pile*.
Battery of the nerves of living animals, projected, i. 187
Belt, galvanic; account of one and its effects, made and worn by Mr. Teed, of Lancaster-court, Strand, - - - - i. 434-437
Bile, operation of galvanism on, - i. 462
Biot, M. his memoir on galvanism, - ii. 154-171
Birds, experiments on the revival of, - i. 299, 300
Blisters. See *Vesicatories*.
Blood; phenomenon observed respecting, in experiments on animals deprived of life in a vacuum, i. 41
experiment by Tourdes respecting the vitality of blood, - - - i. 64
Fowler's experiments respecting the comparative operation of narcotic poisons on the blood, or on the muscles and nerves, - i. 158-160
influence of the blood in galvanic operations, i. 331, 333, 334
proportions of charcoal contained in venous and in arterial blood, - - ii. 330
different capacities for heat, in the arterial and the venous blood, - - ii. 403, 404
expansion of venous, and of arterial, blood by heat, at different degrees of temperature, ii. 416-419
chemical changes produced in the blood by the process of respiration, - ii. 419, 420, 422, 423
explained by Dr. Wells, on the principles of optics, - - - ii. 430
microscopic appearance of the globules of the blood, ii. 431
respiration deemed a stimulus to the circulation, ii. 431
hypotheses on the principle of circulation, ii. 432, 433
question of the return of arterial, into the state of venous, blood, - - ii. 436, *(note)* 437
process of the formation of blood, - ii. 438
Blood-vessels not conductors of electricity, i. 47, 48
experiments by Fowler relative to the influence of galvanism on the vascular system, - i. 151, 154
experiments on the arteries, - i. 154-157, 210-212
Bougies, improvement in the structure of, ii. 458
Brain, effect of wounds in this part of a frog, i. 40
experiments respecting the dependence of the heart upon the brain, - - ii. 196-203
respecting the influence which the destruction of the

VOL. PAGE

the vital energy of the brain has on that of the respective organs, - ii. 204

application of galvanism in cases of paralysis arising from a defective energy in the brain, - ii. 444, 445

Brugnatelli, M. his observations on the nature of the electric acid, - - - - ii. 136-145

C

Caloric, system relative to, - - i. 172

effects of its action on the substance of fat, i. 174

See also *Heat.*

Carbon, power of galvanism in detecting the existence of this principle in substances, ii. 329, 331, 332

Carlisle, Mr. his experiments and inquiries, ii. 46

Carpue, Mr. his experiments, - ii. 104

Cataract, application of galvanism relative to the cure of, - - - i. 419, 465

Celerity of the galvanic and of the nervous fluids, i. 281, 282, ii. 113, 118, 119

See also *Velocity.*

Chain, galvanic: experiments with a chain formed of persons, - - i. 32

communication formed of the hands of the operator, i. 32

effects of different modifications and circumstances of the galvanic chain, upon the fluid, i. 349-360

means to augment the effect of different chains, i. 360

Chapelet de tasses, a peculiar apparatus of **Brugnatelli**, described, - - ii. 139

Charcoal, its power as a conductor of electricity, i. 62, 63, 327

most powerful when recently made, i. 329

as a galvanic conductor, - ii. 84, 183, 184, 326

its conducting power equal both in electricity and galvanism, - - - ii. 330

its different capacity, as a conductor of electricity or otherwise, in an entire and in a powdered state, ii. 217

method of preparing the best charcoal, - ii. 330

attentions in the process, and choice of woods, ii. 331

proportions of charcoal contained in venous, and in arterial, blood, - - ii. 330

See also *Wood.*

Chemical substances, observations of **Pfaff** relative to their effect as members of the galvanic chain, i. 330

Chemistry; discoveries to which galvanism has given, and may give, rise in, - ii. 154, 294-299

— powerful

VOL. PAGE
— powerful effect of galvanism as a chemical agent, - - (note) ii. 394
Coatings, defined, - - (note) i. 198
experiments of M. Valli, on the influence and effects of metallic coatings, - i. 28
effects of the communication between an armed and an unarmed nerve, - - i. 35
a nerve armed at different intervals of the muscle, - - - i. 35
the coating applied to the tunics of the nerves, i. 45
coatings of the muscle, - - i. 46
remark on the application of coating to the nerves, i. 52
observations of Reinhold respecting the application and comparative efficacy of coatings in general, - - - i. 203,204
of homogeneous coatings, - i. 204,205
this last subject treated by Humboldt, i. 272,273
contractions of the muscles produced by coatings of the nerves only, - - i. 212,213
of the muscles only, - - i. 213
by coatings added to moist conductors contiguous to the animal, - - i. 214
different states of the coatings, in different cases, in this experiment, - - i. 215
discovery by Humboldt respecting the coatings of the nerve and muscle, - - i. 274
uses and advantages of metallic coatings to electric jars, explained, - - ii. 269
See also *Metals.*
Cold-blooded animals; convulsions excited in, by the influence of warm-blooded animals, by Aldini, i. 391,394
Combinations, of metals in the construction of the Voltaic pile, - - - ii. 97,100
simple galvanic combinations, - ii. 300-306
theory of galvanic combinations, - - ii. 320
combinations produced by animal substances, ii. 333-342
Condenser; application of, to the electroscope, ii. 344
Conductors, observations respecting the power of different substances as, - - i. 62,63
those of silver recommended by Valli as most powerful, - - - i. 38
effect of a covering of mercury on a metallic conductor, - - - i. 74

VOL. PAGE

— observations of HUMBOLDT respecting an unsound piece of flesh employed as a conductor, and respecting the employment of different fleshy parts, i. 271, 272
living bodies not sufficient conductors without arming, i. 78
imperfection of the human body as a conductor, ii. 315, 363, 364
mushrooms ascertained by HUMBOLDT to be perfect conductors, i. 279
influence of charcoal as a conductor: (See *Charcoal*), ii. 327
  of minerals, ii. 328
  of the Lydian stone, ii. 329
proportionate conducting powers of water, and metallic bodies, ii. 362
all substances pervaded with carbon, become galvanic conductors, ii. 329, 331, 332
all substances, conductors of electricity, likewise conductors of galvanism, ii. 306
conductors, and non-conductors, ii. 214
  table of, ii. 307
effects of different changes suffered by conducting bodies, ii. 215
  by metallic substances, ii. 216
  by water, ii. 216, 217
  by wood, ii. 217

General remarks respecting conductors, by M. VALLI, i. 57, 58
  by BERLINGHIERI, i. 72
divided by VOLTA into *dry*, and *moist*, i. 100, ii. 308
  his various methods of connecting them, i. 101
  another combination, i. 102
  each piece of metal insulated between aqueous conductors, i. 103
    illustrated, i. 104
  combinations of two pieces of metal with conductors of different kinds, i. 105
  when connected in a circle, i. 105
  moist conductors described, i. 106
    experiment, i. 106
  experiment; a moist conductor between two different metals, i. 108
    between two pairs, i. 109
— conclusion

VOL. PAGE

- — conclusion from the combinations, i. 109
  - simple cases of connexion, - i. 110
    - two different *dry* conductors in contact, applied on the other side to *moist* ones, - i. 110
    - a metal between two different *moist* conductors, i. 111
    - experiment, and inference, - i. 112, 113
  - classification of *moist* conductors, - i. 113
    - ordinal arrangement, relative to their proportional power, - - - i. 114
  - comparative fitness of different metals, in their application between these conductors, i. 115
  - case of a bow, consisting of one metal, touching with both its ends the same species of fluid, i. 116
    - experiment, - - i. 117
  - substances proper for comparative experiments of this kind, - - i. 118
  - metals proper to be used, - i. 119
    - superior advantages of tin, - i. 120
    - fluids best adapted to be applied to it, i. 120
  - experiment with two metals of a different kind, and a fluid, - - - i. 121
  - manner of exciting the electric fluid, by a circle of three different moist conductors, - i. 122
    - experiment, - - i. 123
  - conclusion, importance of conductors in the theory of galvanic operations, - i. 124
  - additions to these observations: experiments by the assistance of Nicholson's doubler, i. 125-132
    - conclusion: the mutual contact of two different metals, the essential cause of excitiug the electric fluid, - - - i. 133
  - contractions produced by coatings added to moist conductors contiguous to the animal, i. 214, 215
  - experiments with various fluid conductors, by Reinhold, - - i. 217, 218
  - experiments through the agency of heat, as in glass, ii. 218
    - by violent pressure, - ii. 218, 219
  - different states of electricity exhibited by an insulated conductor, - - i. 249
    - illustration, - - i. 250
      - Respecting the conductors employed in the galvanic pile—See *Pile*.
- *Copper* (or *Brass) leaf*; deflagration of, by the galvanic battery, - - ii. 393

VOL. PAGE

Cortambert, his objections to the theory of Galvani, - - i. 75-77

Couronne de tasses; construction of, in the formation of the Voltaic battery, - ii. 352

Cramp, application of galvanism to the relief of this affection, - - - ii. 456

Creve, his discovery on metallic irritation, i. 311

Cruickshank, Mr. his experiments and inquiries with the galvanic pile, - - ii. 52

Crystals (electric) of silver described, - ii. 140
 of tin, brass, and steel, - - ii. 141, 142
 attentions in producing these crystallizations, ii. 141

Cuvier, Professor; report on galvanism, made by him to the French National Institute, ii. 145

D

Davy, Mr. his observations and experiments with the galvanic pile, - - ii. 80

Deafness, application of galvanism in the cure of, i. 422, 427, 437, 443
 mode of operation, - - i. 432, 433
 in the Author's practice, - - ii. 449-454

Decomposition of concentrated sulphuric acid, ii. 69
 of pure and colourless nitric acid, - ii. 69
 of water. See *Water*. - -

Deflagration of metals: of gold-leaf, - ii. 64
 other experiments respecting deflagration, ii. 65
  gold, silver, copper, - - ii. 66
  lead, tin, zinc, - - ii. 67
  gold, silver, brass, copper, zinc, tin, ii. 134
  finely-drawn metallic wires, - ii. 392
  finely-laminated metals; gold-leaf, silver-leaf, copper (or brass) leaf, tin-foil, - ii. 392, 393
 this operation performed in a vacuum, ii. 394
  in carbonic acid gas, - - ii. 394
  by the electrat produced in the galvanic decomposition of water, - - ii. 394
  on quicksilver, - - ii. 395
 comparative influence of electricity and galvanism in this process, - - ii. 391

Diaphragm of a dog, experiment on, i. 58
 of horses, - - - i. 60
 extreme sensibility of this membrane, (*note*) i. 176
 effects of galvanism upon, in warm-blooded animals, i. 265

VOL. PAGE

Doubler; experiments made with, relative to conductors, and their influence upon the connexion of metals, - - i. 125-132
application of, to the electroscope, - ii. 345
Drowned animals, experiments on, - i. 49
See also *Suspended Animation.*

E

Eggs: theory of the causes producing the discoloration of silver spoons employed in the act of eating eggs, - - - ii. 323
Electricity: phenomenon of *animal electricity* first observed by GALVANI during an electrical operation, - - - i. 6, 7
his electrical experiment on this subject, i. 8
his application of the electricity of lightning to the muscular system, - - i. 9
electrical tension produced by different metals, ii. 31, 33
conjectures respecting a new electro-motor, ii. 35
experiments proving the effect of oxydation in the production of electricity, - ii. 86-96, 120-125
metallic oxyds produced by electricity, ii. 390
remarks upon conductors and non-conductors: (See *Conductors)*, - - ii. 214
positive and negative electricities not two distinct principles, - - - ii. 220
emission of positive and negative signs by different substances, on excitement, - ii. 221
different capacities of substances to retain the electric fluid, - - ii. 222
principle of the admission of the electric fluid into a non-electric body, caused by attrition, ii. 224, 226
electric properties of glass in different methods of excitement and application, - ii. 226
best sort of amalgams, - (note) ii. 231
influence of electricity on air, - ii. 233
influence of points on electricity, explained, ii. 246
different states of electricity exhibited by an insulated conductor, - - ii. 249
illustration, - - - ii. 250
illustration of the quantities of the electric fluid contained by electrics, - (note) ii. 257
theory of the Leyden phial, - ii. 259
illustration, - - - ii. 261, 263
theory of spontaneous discharges, - ii. 265
uses and advantages of metallic coatings, ii. 269

VOL. PAGE
— theory of electric phenomena, - ii. 273
effects of the resistance which bodies in their different states furnish to heat, - ii. 273
varying state of the atmosphere, - ii. 274
influence of light on the electric capacities of bodies, - - - ii. 276
production of various atmospherical meteors: (See *Meteors)*, - - ii. 276
arrangement of electricity, magnetism, and caloric, - - - ii. 282
consequence, - - ii. 283
polarity imparted to needles by electricity, ii. 283
phenomena of water-spouts, - ii. 285-289
of fairy-rings, - - *(note)* ii. 288
of electrical animals, - - ii. 396

Question of the Connexion of the Galvanic and Electrical Principles.

Their identity proved by M. DE LA METHIERE, i. 64
observations of M. BERLINGHIERI, in support of this opinion, - - - i. 72
possible objections, - - i. 77
opinion of their identity supported by VASSALLI EANDI, - - - i. 80-88
examined by FOWLER, - - i. 136, 137
their dissimilitude proved by him, i. 142, 143, 160, 161
opinion of Dr. WELLS, - - i. 163-165
their identity denied by REINHOLD, - i. 255
by HUMBOLDT and CORRADORI, i. 263, 297, 298
by FABRONI, - - i. 316
their analogy asserted, in the report made to the French National Institute, by M. HALLE, i. 384
doubted in a subsequent report, by Professor CUVIER, - - - ii. 147
asserted by the theories of VOLTA, - ii. 9, 24
opinion of M. DUMAS, - ii. 187
of the Author, - - ii. 213
review of different theories respecting their connexion:
their similarity now generally acknowledged, ii. 290-292
distinction which may be admitted, ii. 302
identity of the electric and the galvanic fluid proved by Mr. NICHOLSON, - ii. 50
by the experiments of VAN MARUM and VOLTA, ii. 112
opinions respecting, founded on the experiments of the naturalists of Berlin, - - ii. 132-134

—Inferences

VOL. PAGE

—Inferences of M. MANDUYT, relative to the dependence of galvanic operations upon electricity, i. 36
theory of VOLTA on the subject, - i. 94, 96
comparative trials between the galvanic and electric phenomena, - - i. 380
comparative action of electricity and of galvanism, when applied to medicine, i. 450, 451, ii. 442-447
explanation of the medical action of electricity upon the human body, - - i. 456
phenomena of apoplexies and epilepsies, compared by GALVANI with the effects of electricity, i. 401, 402
his opinion on the operation of electricity as a remedy, and on the mode of administering it, i. 403, 404
comparative influence of electricity and of galvanism in the deflagration of metallic substances, ii. 391
decomposition of water by electricity, ii. 380
elucidated in its decomposition by galvanism, ii. 382
analogy of the stimulating power of animal to that of other electricity, - - i. 26
state of electricity in an animal during galvanic operations, - - - i. 34
effect of the application of electricity to an animal during galvanic operations, - i. 74
quantity of electric fluid put in motion by the Voltaic apparatus, larger than can be obtained by the common electric machines, - ii. 20
See also the articles, *Fluids*, *Leyden phial*, and *Torpedo*.
Electroscopes and *electrometers* explained, ii. 236
electrometer of CUTHBERTSON described, ii. 242
the *electrophorus* explained, - ii. 253
gold-leaf *electroscope* of Mr. BENNET, ii. 244, 313
addition of the galvanometer, - ii. 343
of the condenser, - - ii. 344
illustrations, - - ii. 345, 346
Epidermis; experiments by HUMBOLDT, respecting its effects as a part of the galvanic chain, i. 277
its tendency to weaken or intercept the effects of galvanism, - - - i. 368, 371
Epilepsies; phenomena of, compared by GALVANI with the effects of electricity, i. 401, 402
Excitement; definition and classification of, by REINHOLD, - - - i. 253
galvanism a method of estimating the degree of excitement of a nerve or muscle, i. 420
excitement produced without the aid of metals, ii. 333-342
See also *Incitability*.

VOL. PAGE

Exciting arc. See *Arc.*

Experiments, curious observation of VALLI respecting the performance of, - - i. 53
  unnecessary cruelty to be avoided in experiments on animals, - - - - i. 65
  observations respecting attentions necessary in performing experiments, - - i. 375, 376
    particularly when made for the purpose of forming a theory, - - i. 190

F

FABRONI, his experiments and observations respecting the operation of metallic substances, i. 313

Fairy-rings, annular marks on the grass so called; produced by a discharge of electric matter, *(note)* ii. 288

Falling stars. See *Shooting stars.*

Feeling; experiments respecting the influence of galvanism on, - - i. 223

Feet of several animals; peculiarities of, in galvanic operations, - - i. 57

Fibres; the existence of a sensible and irritable one, necessary to produce in animals the galvanic phenomena, - - i. 226
  result of the experiments of HUMBOLDT to ascertain the existence of sensible fibres in the tendons and ligaments, - - - i. 279
  effect of a division of the sensible fibres, i. 287

Fire-balls; production of, dependent on electricity, ii. 278

Fishes, effects of galvanism on, - i. 291
  experiments on the hearts of fishes, - i. 304
  structure of their gills, - - ii. 434
    of the air-bladder, - - ii. 435

Flesh; observations of HUMBOLDT respecting the employment of different pieces of, as conductors, i. 271, 272

Fluids: considerations on the nature of the *galvanic fluid*, by REINHOLD, - - i. 252
    inquiry whether properly intitled *fluid*, i. 254
    whether it is secreted in the animal, i. 255
    conclusion respecting its nature, - i. 258
  his division of the hypotheses of different authors respecting the galvanic fluid, - i. 258, 259
  the galvanic fluid analogous, but not identical, with magnetism and electricity; asserted by HUMBOLDT, - - i. 263
    his further remarks respecting galvanic fluid, i. 266
    idea of the nature of the galvanic principle, and its analogy with electricity, deduced from his experiments, - - i. 383, 384

VOL. PAGE

— theory of LEHOT respecting the nature and currents of the galvanic fluid, i. 340-362
facts and inferences relative to the operation of the galvanic fluid by ALDINI, - i. 388, 389
theory of Dr. PRIESTLEY, respecting the galvanic fluid, - - - ii. 77
rapidity of the galvanic current, ii. 113-118, *(note)* 391
and also of the *nervous fluid*, - i. 281, 282
experiments, - - ii. 119
identity of the *nervous* and the *electric* fluid proved by experiments, - i. 44, 77
denied by CORTAMBERT, - i. 77
chemical observations of BRUGNATELLI on the nature of the *electric fluid*, - ii. 136-145
effects of the employment of different fluids in the composition of the Voltaic pile. See *Pile*.
FONTANA, several experiments by, - i. 60
FOWLER, his experiments and observations, i. 134-160
Frogs: these animals peculiarly adapted for galvanic experiments, - - i. 292
extreme sensibility of their muscular fibres to the influence of electricity, - ii. 313, 316
periods of their greatest susceptibility, i. 383
species of them more particularly subjected to experiments, - - i. 199
methods of preparing them, i. 200, ii. *(note)* 303
time which they are able to support galvanic operations, - - i. 38, 39, 206, 207
experiments to revive some killed by asphyxy, i. 50
causes of variety in the results of experiments made upon these animals, - i. 9
Fungi, observations on their galvanic properties, ii. 339, 340
See also *Mushrooms*.

G

GALVANI, succinct account of his life, i. 4
the incident which led him to the discovery of galvanism, - - i. 6
his experiments, - - i. 7-27
his hypothesis, - - i. 12, 13
Galvanometer, description of, - ii. 343
Gas; observations respecting the production of, in experiments with the galvanic pile, - ii. 47, 48, 50
in the decomposition of water, - ii. 51
other experiments on the subject, - ii. 52
conclusions, - - ii. 58

— further

VOL. PAGE
— further experiments respecting the production of gas, - - ii. 81, 102, 152
by the communication of the two extremities of the galvanic pile with water, - ii. 149
hypotheses in explanation of this phenomenon, ii. 150
inquiries whether the galvanic influence can be transmitted through the gases, - ii. 70
gases produced from different chemical mixtures, ii. 71,73
Glass; its capacity, in different states, as a conductor of electricity, or otherwise, - ii. 218,221
its electrical properties in different methods of excitement and application, - ii. 226-231
best sort of amalgams, - (*note*) ii. 231
its effect on the passage of the electric fluid, i. 74
question of its permeability by electricity, (*note*) ii. 259
permeable, when thoroughly heated, by animal electricity, - - i. 57
theory of the Leyden phial, and of electrical jars, ii. 259-268
uses and advantages of metallic coatings, ii. 269-272
Gold-leaf; deflagration of, by the galvanic pile and battery, - ii. 64, 66, 134, 392
GRAPENGIESSER, Dr. his work on the application of galvanism to medicine, - i. 422
Gutta serena, application of galvanism in the cure of, - - i. 427, 431, 452, ii. 455
Gymnotus, electrical properties of, - ii. 397

H

HALDANE, Colonel; his experiments and inquiries with the galvanic pile, - - ii. 96
HALLE, M. his Report to the French National Institute, - - i. 363
method of exciting the galvanic phenomena in animals, - - - i. 363,364
arrangement of the heads of inquiry, - i. 364
experiments and conclusions respecting the essential parts of the animal arc, and their dispositions, i. 365,366
respecting the parts of the exciting arc, and their relative nature and dispositions, - i. 368
extraneous causes having an influence on the experiments, - - - i. 372
reflections and conclusions, - i. 374
experiments respecting the means of varying the susceptibility of animals, - i. 376
comparative trials between the galvanic and electric phenomena, - - i. 380

VOL. PAGE

— experiments made by HUMBOLDT in presence of the members of the commission, i. 382
general deductions on the nature of the galvanic principle, and its future application to medicine, i. 383
Hearing; experiments respecting the influence of galvanism on this sense, - i. 149, 223, ii. 6
respecting its influence in the cure of deafness and of particular buzzings in the ear. See *Deafness*.
Heart, question of the action of galvanism upon; experiments with the hearts of different animals, i. 60, 61, 147, 208, 209, 303-305, 333, 334, ii. 465
remarks respecting the foramen ovale, - ii. 427
effect of the oxygenated muriatic acid upon the heart, - - i. 170
experiments respecting the dependence of the heart upon the brain, - - ii. 196-203
Heat; question of the influence of the degree of temperature upon the susceptibility of metals to the galvanic principle, - - i. 246
changes effected by heat, in the electric properties of bodies, - - ii. 218
question of the mechanical diffusion, or the chemical union, of caloric, relatively to matter, *(note)* ii. 219
principles of the general diffusion of caloric in substances, - - - ii. 224, 225
beneficial effects of the resistance which bodies in their different states furnish to heat, - ii. 273, 274
arrangement of caloric with electricity and magnetism, - - ii. 282
Dr. CRAWFORD's experiment on the powers of atmospherical and pure air to retain heat, ii. 404-407
his calculation of the comparative heat of common and of fixed air, - - ii. 408
of inflammable air and vapour, - ii. 408
of inflammable and common air, - ii. 409
of the capacity (for heat) of water and of snow, ii. 409
experiment of the Author on this subject, ii. 410
of the capacity of vapour, - ii. 411
experiments, - - ii. 412
expansion of different substances at different degrees of temperature; mercury, common air, and water, - - ii. 415
venous, and arterial, blood, - ii. 416-419
experiments and remarks relative to the temperature of the human body, - - ii. 424-428

VOL. PAGE

——it is not dependent on respiration, ii. 429

Hemiplegia, application of galvanism in the cure of, i. 407, 453, ii. 444, 446

HENRY, Mr. of Manchester, his experiments and observations with the galvanic pile, - ii. 68

Hoarseness, application of galvanism in the cure of, i. 428, 434

Homogeneous coatings; effect of, as described by REINHOLD, - - - i. 204, 205

by HUMBOLDT, - - i. 272, 273

See also *Metals*.

homogeneous metals may be used in the construction of the Voltaic pile, - ii. 85

HUMBOLDT: his experiments to ascertain the degree of irritability inherent in an animal, i. 167, 168

General statement of his experiments and observations on galvanism, - i. 261

conclusions drawn by him from his early experiments, - - i. 265

his project of forming a chemical analysis of the principle of vitality, - i. 266

his new ideas on the subject of irritability, i. 267

his labours in the development and prosecution of these, - - i. 267, 268

observations respecting the relation between galvanic irritation and incitability, - i. 269

means by which he increased the incitability of the animals, - - i. 270

experiments respecting galvanic irritation produced without a coating, or metallic or charcoal substances, - - i. 270

remarks on the employment of different pieces of flesh as conductors, - i. 271, 272

on the excitement produced by a simple metallic substance, or homogeneous metallic parts, i. 272

experiments, - - i. 273

heterogeneous metals, - i. 274

remarks respecting the classification of substances, i. 275-277

experiments respecting the comparative effects of animal and vegetable substances employed in the galvanic chain, - - i. 277

respecting the phenomenon of *setting the teeth on edge*, - - - i. 278

——respecting

VOL. PAGE
——respecting the existence of sensible fibres in the tendons and ligaments, i. 279
experiments on mushrooms, ascertaining their fitness for conductors, i. 279
remarks respecting the length of conductors, and the celerity of the transition of the galvanic influence, - - i. 280, 281
influence of the state of the atmosphere upon the nerves, as upon a hygrometer, i. 282
experiments on the tongue, and on a nerve, which thus distinguish metals, - i. 283
the muscular movements and sensations produced by galvanism, prolonged after the chain has been closed, - - i. 284
effect of ligatures, and of division of the fibres, i. 286, 287
of different *media*, - - i. 288
effects of galvanism upon vegetables, i. 289
upon aquatic worms, - i. 290
upon insects, - - - i. 290
upon fishes, - - i. 291
upon amphibious animals, - i. 291
upon man, - - - i. 293
in the sense of sight, - - i. 293, 294
of taste, - - - i. 295
of smelling, - - - i. 296
in producing inflammations, - i. 296
on vesicatories, - - i. 297, 298
experiments on the revival of birds, i. 299, 300
on the peristaltic movements of the intestinal canal, - - - i. 300
on the hearts of different animals, i. 303-305
General deductions respecting the established theories of galvanism; that of GALVANI, i. 305, 306
of VOLTA, - - - i. 306, 307
theory of HUMBOLDT, - i. 308
experiments made by him in the presence of the commissaries of the French National Institute, i. 382
his experiments and observations respecting the application of galvanism to medicine, i. 408-421
remarkable instances of his courage and resolution in the pursuit of his philosophical researches, i. 284

Hydro-

VOL. PAGE
Hydrophobia, application of galvanism in the cure of, - - (*note*) ii. 462
remarkable case, - - i. 454-456
Hypochondriasis, application of galvanism in the cure of, - - - ii. 462

I

Ice. See *Water*.
Incitability, defined by Reinhold, - i. 228
different degrees of in different animals, according to that naturalist, - i. 228, 229
after death, - - i. 230
this quality increased by inflammation, i. 230, 231
species of incitability resulting from different deaths, - - - i. 231
incitability artificially exalted; by opinion, i. 232
by the oxygenated muriatic acid, animal calxes, and alkalis - - i. 233
by moistening the operated nerve with water, i. 233
by galvanism itself, - - i. 234
incitability depressed by a natural mode; physiologically, - - i. 234
pathologically; by the mechanical injury of the nerves and muscles, - i. 235, 236
by certain diseases, and kinds of death, i. 236, 237
incitability depressed by an artificial mode; with chemical applications, - i. 237
arrangement observed in these remarks respecting the modifications of incitability, - i. 238
Humboldt's observations on the relation between galvanic irritation and incitability, i. 269
means by which he increased the incitability of the animals, - - - 270
See also *Excitement*, and *Irritability*.
Inflammations, effects of galvanism in producing, i. 296, 297
Insects, action of galvanism upon, - i. 290
Intestinal parts, experiments respecting, i. 299-303
Irritability; Humboldt's experiments to ascertain the degree of this property inherent in an animal, i. 167, 168
effects of the oxygenated and the simple muriatic acid in restoring irritability, - i. 168-170
——deductions

VOL. PAGE

——deductions relative to the nature of irritability, - - i. 171
distinction of irritability from sensibility, i. 181
irritability in the limbs of animals, supported, not destroyed, by excitement, - i. 59
See also *Excitement*, and *Incitability*.
Irritation of the muscular fibre, the foundation of the science of galvanism, - - i. 261, 262
researches of HUMBOLDT into the principle and theory of irritation, - - i. 267
his experiments respecting galvanic irritation produced without a coating, or metallic or charcoal substances, - - i. 270
effects of the galvanic irritation totally dissimilar to those of the electric shock, - i. 297, 298
discovery of CREVE on metallic irritation, i. 311-313

L

Lead, various examples of the influence of this metal as a coating, - - - i. 28-35
as an exciting arc, applied to zinc, i. 32
Leeches, experiments on, - - i. 146
LEHOT, M. his observations and experiments, i. 340
Leyden phial, experiments of ALDINI to prove its similarity to the Voltaic pile and to animal substances, - - - i. 392
the Leyden phial charged by the Voltaic apparatus, ii. 18
experiment with the Voltaic pile connected with the Leyden phial, - - ii. 98, 99
analogy between the Voltaic pile and the Leyden phial, as stated by Professor CUVIER, ii. 146
theory of the Leyden phial, - ii. 259
illustrations, - - ii. 261, 263-265
Ligatures; inquiries respecting their effects on the nerves, in impeding the progress of the galvanic current, i. 34, 35, 39, 58, 77, 186, 286, 287, ii. 340
experiments of REINHOLD, - i. 206
Light, its influence on the electric capacities of bodies, ii. 276
nature of its general motion, - ii. 348
—production

VOL. PAGE
—production of the flash of light by the Voltaic pile, - - - ii. 6
See also *Sight*.
Lightning; application of its electricity to the muscular system, by Galvani, - i. 9
Locked jaw. See *Tetanus*.
Lungs; the structure of, similar to the torpedinal arrangement (see ii. 398, 399), ii. 402
vascular formation of the lungs, - ii. 433
See also *Respiration*.
Lydian stone, its property as a conductor, ii. 329

M

Magnetism, inquiry respecting its connexion with galvanism, - - i. 144, 254
its arrangement with electricity and caloric, ii. 282
See also *Needle*.
Media; four different kinds of functions resident in some, according to Reinhold, - i. 224, 325
inquiries of Humboldt respecting the effects of different media, - - i. 288
effects of different media on the susceptibility of the animal, - - - i. 373, 374
Medicine: Galvani endeavours to apply his discovery to practical medicine, - - i. 15
possible benefits to be derived from its application to that science, - - i. 385, 386
attempts of Aldini towards this purpose, i. 393
Review of the most interesting attempts which have been made to apply the principle of galvanism to the cure of diseases, - - i. 396
obstacles hitherto experienced in these endeavours, i. 396, 397
hypothesis of Galvani relative to the production of the nervous sciatica, - i. 398
of the tetanos, and of paralysis, - i. 400
employment of galvanism to distinguish real death from apparent or asphyxy: (See *Asphyxy*), i. 406
its application proposed in the paralysis of the optic nerve; successful treatment of a case of hemiplegia, - - i. 407
Humboldt's examination of the benefits to be ex-

VOL. PAGE

pected from the application of galvanism to the science of medicine, - i. 408
question of the efficacy of galvanism in paralytic diseases: (See *Paralysis*), - i. 417
Galvanism a method of distinguishing the nerves from the other organs, and of estimating the degree of excitability of a nerve or muscle, i. 420
work of GRAPENGIESSER on the employment of galvanism in the treatment of several diseases; substances most proper for the materials of the galvanic pile, - - i. 422
poles of the pile, and their respective electricities, i. 423
varieties in the effects of the pile and the battery, i. 424-426
particular maladies in which galvanism may be resorted to: palsies of the extremities; debility of sight, and the gutta serena; deafness, and buzzings in the ears, - i. 427
hoarseness, and aphony arising from a defect of nervous action, - - i. 428
paralysis of the sphincter ani, and of the urinary bladder, asphyxy, and various other cases, i. 429
modes of application of the battery in different diseases; in paralysis of the extremities, and to the wounds of blisters, - i. 430
in diseases of the sight, - i. 431, 432
of the hearing, - - i. 432, 433
in paralysis of the urinary bladder; hoarseness, aphony, &c. and chronic rheumatisms, i. 434
account of a galvanic belt made and worn by Mr. TEED, of Lancaster-court, Strand, i. 434
method of administering in cases of deafness, i. 437
experiment in a violent paralysis of the face, i. 438-443
in a case of deafness, - i. 443
remarks on these applications of galvanism to the human body, - - i. 443
experiment in a case of paralysis, i. 444
substance of a report by VASSALLI-EANDI on the application of galvanism to medicine, i. 449
experiment in a case of gutta serena, i. 452
of hemiplegia, - - i. 453
of hydrophobia, - - i. 454

—instances

VOL. PAGE

—instances of misfortunes occasioned by the abuse of galvanism, - - i. 460, 467

observations of M. Desmortiers on the danger of galvanism in the treatment of diseases, i. 461

experiments on the bile, - i. 462

on the temples, - - - i. 463

remarks, - - - i. 463

effect of its application to the urinary calculus, i. 463, 464

reflections, - - - i. 466

Attentions in the medical application of galvanism, ii. 192, 193

idea of the Author, relative to its employment in medical practice, - - ii. 213, 214

Cases of its application to medicine, in the Author's experience ; in paralytic affections, ii. 444-446

attentions in the use of the battery, ii. 447

remarkable cure of paralysis, - ii. 448

cases of deafness, - - ii. 449

different methods to be pursued, according to the different sources of this defect, ii. 449

mode of the application of galvanism described, ii. 452

when one ear only is affected, - ii. 454

cases of weakness of sight, and of amaurosis, ii. 455

in spasmodic affections ; cramp, tetanus, and stiffness of the joints, - ii. 456

indolent tumors, scrophulous swellings, and enlargements of the prostrate gland, ii. 457

cases of mental derangement ; melancholy madness, ii. 459

instance of relief experienced, - ii. 461

hypochondriasis, - - ii. 462

female complaints ; nervous headachs, ii. 463

cases of suspended animation, - ii. 463

method of operation, - ii. 468

Comparative effects of galvanism and of electricity, when applied to medicine, i. 450, 451, ii. 442

explanation of the medical action of electricity on the human body, - - i. 456

opinion of Galvani relative to the operation of remedies, and the manner of administering electricity, - - - i. 403

—superiority

VOL. PAGE
— superiority of the galvanic fluid to that the of common electricity in certain cases, in determining the nervous action, - i. 423, 424
apoplexies and epilepsies compared by GALVANI with the effects of artificial electricity, i. 401
Mental derangement, application of galvanism in the cure of, - - ii. 459-463
Mercury. See *Quicksilver*.
MESMER, observations upon his system of magnetism, ii. 211, 212
Metals: their influence upon the movements of the muscles, discovered by GALVANI, i. 10
action of the heterogeneous and homogeneous metallic coatings upon the electricity in living bodies, investigated, - i. 23, 24, 72, 73
of lead and silver, - - i. 28
the lead removed, - - i. 29
of similar metals: different positions of the coatings: various coatings applied to the nerves of the thighs, - - i. 29
lead; with itself, and with other metals, i. 30
inferior power of the lead: influence of different metals combined with it: effect of the leaden arc applied to zinc, - i. 31
further examples of coatings of lead, i. 33
phenomena in the operation of different metals, as coatings, or as exciting arcs, i. 45
effect of a covering of mercury on a metallic conductor, - - - i. 74
galvanic capacity of different metals, as observed by GAILLARD, - - i. 79
theory of VOLTA, explaining the operation of metals in galvanic processes, - i. 95, 96
observations by FOWLER, respecting the choice and use of metals in experiments, i. 138
respecting metallic oxydes, - i. 139
contractions excited by a single metal, with friction, - - i. 163
modification of the galvanic forces of metals; produced by different degrees of temperature, i. 245, 246
by the friction of certain bodies, i. 246, 247
discovery of CREVE, on metallic irritation, i. 311-313
hypothesis of FABRONI respecting the action of metals, - - i. 314-317

VOL. PAGE

—— on amputated limbs, in different cases, ii. 191

extreme sensibility of the muscular fibres of a frog to the influence of electricity, - - ii. 313-316

See also *Nerves*.

Mushrooms, ascertained by HUMBOLDT to be perfect conductors, - - i. 279

See also *Fungi*.

N

Needle, magnetic; variation of, - (*note*) ii. 280

polarity imparted to needles by electricity, ii. 283

Nerves: not conductors of the electric fluid, i. 47, 48

the nervous fluid that which is acted upon by poisons; inferences, - - i. 52, 53

part of the nerves which longest retains vitality, i. 55

opium more powerful when applied to the extremity than to the origin of the nerves, i. 60

experiments concerning the source whence the respective forces of the nerves and muscles emanate, - - i. 152, 153

system of the reproduction of the nerves established, - - i. 157

the nervous system distinct from that of vitality, - - i. 175-178

the former merely an appendant to, and a shortener of, life, - - i. 178

remarks of M. SUE respecting the true seat of sensation, - - i. 182, 183

experiments, - - i. 184

influence of the state of the atmosphere upon the nerves, as upon a hygrometer, i. 282

remarks on the infinitude of sensations, (*note*) ii. 342

Electrical phenomenon arising from the nervous system observed prior to the discovery of GALVANI, - - i. 3

hypothesis of VASSALLI on the subject of this experiment, - - i. 3

communication between an armed and an unarmed crural nerve, - - i. 35

experiment on a nerve armed at different intervals, - - i. 35

attempted on a hot-blooded animal, i. 36

VOL. PAGE

— manner of operation of the coating applied to the tunics of the nerves, - i. 45

effects of the action of nitrous gas on the nerve of a frog, - i. 52

remark on the application of coating to the nerves, - - i. 52

contractile movements produced by the experiments of BERLINGHIERI, - i. 67-69

in hot-blooded animals, - i. 70

inquiry by FOWLER, whether the nerves *in general* are equally subject to the influence of galvanism, or those only which are governed by *the will:* (See *Will*), - i. 147

respecting the direction followed by the galvanic influence when applied to the nervous system, - - - i. 152

experiments on the arteries, - i. 154-156

different effects of galvanism when applied to a particular nerve, or to the brain or spinal marrow, - - i. 160

experiments by REINHOLD relative to the external senses, - - i. 219-224

conditions connected with the nervous system, necessary to produce in animals the galvanic phenomena, - - i. 226, 227

incitability not particularly diminished by the mechanical injury of the nerves and muscles, i. 235, 236

remarks upon living nerves and muscles, by HUMBOLDT, - - i. 265, 266

respecting communications between two points of the same nerve, - i. 266

his discovery respecting the coatings of the nerve and muscle, - - i. 274

his experiments on a nerve, which thus distinguishes metals and charcoal, - i. 283

experiments and conclusions respecting the animal arc by HALLE, - - i. 366, 367

galvanism a method of distinguishing the nerves from the other organs; and of estimating the degree of excitability of a nerve or muscle, i. 420

superiority of the galvanic fluid to that of common electricity in certain cases, in determining the nervous action, - - - i. 423, 424

opinion of ALDINI, that the galvanic fluid is inherent

VOL. PAGE

in the animal fibre alone, refuted, ii. 316, 317
experiments of excitement produced without the aid of metals, - - ii. 333-335
question of the dependence of galvanism on the nervous influence, - ii. 338, 339
effects of dividing a nerve, - - ii. 341
battery of the nerves of living animals projected, i. 187
See also *Fluids*, *Ligatures*, and *Muscles*.
Nervous headach, female complaints; application of galvanism in the cure of, - ii. 463
NICHOLSON, Mr. his reply to the hypothesis of VOLTA respecting the galvanic pile, - ii. 22
his experiments and inquiries, ii. 46

O

Oil, effects of experiments in, - i. 74
Opium, more powerful when applied to the extremity than to the origin of the nerves, i. 60
experiments respecting its power in causing incitability, - - i. 232
See also *Poisons*.
Oxyds, definition of the term, (*note*) i. 239
process and effect of oxydation in electricity and galvanism, - ii. 86-96, 120-125, 308-312
Oxygen, experiments relative to its absorption by the galvanic pile, - - ii. 171-175
influence of oxygen gas on the maintenance of animal life, - - ii. 421

P

Paralysis, hypothesis of GALVANI respecting its production, - - i. 400
question of the efficacy of galvanism in the cure of paralytic diseases, - i. 417, 418, 422
examples, paralysis of the optic nerve, or gutta serena: (See also *Gutta Serena)*; hemiplegia, i. 407, 452, 453
palsies of the extremities, - i. 427
mode of application of the battery in these cases, i. 430

VOL. PAGE

—— paralysis of the sphincter ani, and of the urinary bladder, - - i. 429

remarkable experiment in a violent paralysis of the face, - - - i. 439-443

another experiment in a paralytical case, i. 444-449

the Author's experience in the application of galvanism to paralytical affections, - - ii. 444

cases of hemiplegia, - - ii. 444-446

cases arising from a defective energy in the brain, ii. 444,445

remarkable cure, - - ii. 448,449

Peristaltic movements of the intestinal canal, experiments respecting, - - i. 300-303

PERKINS, observations upon his system of tractors, ii. 211,212

Perspiration; proportion of, to the food, ii. 424

PFAFF, Professor, opposes the theory of GALVANI, i. 18,20

his experiments and theory, - i. 327

Pile, galvanic; invention of, by VOLTA, - ii. 1

its construction, and manner of application, ii. 2,3

its operation, - - - ii. 4

its effects purely electric, - ii. 5

VOLTA's theory of the pile, - - ii. 5

production of the flash of light, - ii. 6

contrivance to preserve the moisture of the pile, ii. 7

analogy of this apparatus to the electrical organ of the torpedo, - - ii. 8

its phenomena resolved into the electrical principle, - - - ii. 9

experiments inferring this deduction, ii. 10

analysis and illustration of its operation, ii. 14,15

employment of saline fluids in moistening the interposed layers, compared with the effects of pure water, - - - ii. 16,17

Leyden phial, and even large electric battery, charged by the pile, - - ii. 18

attentions in performing the experiments, ii. 19

quantity of electric fluid put in motion by this apparatus, larger than can be obtained by the common electric machines, - - ii. 19

varieties in results, according to different communications of the extremities with the ground, ii. 21

observations by Mr. NICHOLSON in opposition to the theory of VOLTA; effect of the different fluids, ii. 22

new theory of VOLTA, - ii. 24

VOL. PAGE
——electrical tension produced by different metals, ii. 31, 33
ratios subsisting between the conductors of different classes, - - ii. 35, 36
report of a commission of the French National Institute on this last theory, - ii. 38
experiments and inquiries made with the Voltaic pile by British philosophers, - ii. 46
production of gas from the wire used in experiments, ii. 47
in the decomposition of water, - ii. 51
other experiments on the subject, - ii. 52
conclusions, - - ii. 58
experiments respecting acid solutions of metals, ii. 60
experiments with the trough, - ii. 63, 64
the pile may be constructed of homogeneous metals, ii. 85
combinations of different metals, - ii. 97, 100
experiment with the pile, connected with the Leyden phial, - - - ii. 98, 99
experiments and inquiries by the continental philosophers:—of Van Marum, proving that the denomination of the pile should be electric, and not galvanic, - - ii. 107-112
celerity of the current put in motion by the pile, ii. 113, 118, 119
experiments with piles of different constructions, ii. 114, 115
necessary attentions in the nature and degree of moisture of the layers, - ii. 117
experiments with piles constructed of gold and zinc, and of silver and zinc: compared; and combined, ii. 128, 129
augmented effect produced by terminating the extremities of the conductors with a point of iron wire, and a button, respectively, ii. 129, 130
inverse state of the two conductors ascertained, ii. 131
deflagration of various metals, by a pile of 180 layers, - - - ii. 134
analogy between the pile and the Leyden phial, ii. 146
experiments respecting the effect produced by augmenting the surface of each of the discs, ii. 152-154
M. Biot's theory of the operation of the pile, ii. 154, 155
his observations relative to the size of the plates, ii. 156, 157
experiments on this subject, ii. 158, 159, 162

—— effects

VOL. PAGE

—— effects of different arrangements of the strata, ii. 162-164
effects of water (employed as a conductor) on the movement of the galvanic fluid in the interior of the apparatus, - - ii. 164-170
conclusions respecting the laws of the movement of the galvanic fluid, - - ii. 170
experiments relative to the absorption of oxygen by the pile, - - ii. 171-175
remarks on substances most proper for materials of the pile in its application to medicine, - i. 422
poles of the pile, and their respective electricities, i. 423
particular construction of a pile, - (note) i. 444
use of the pile superseded by the improved battery, - - - ii. 351,357
varieties in the effects of the pile and the battery, i. 424-426
employment of the pile in medicine. See *Medicine.*
See also *Battery.*

Plants. See *Vegetables.*

Points; influence of, on electricity, explained, ii. 246-249

Poisons: Fowler's experiments respecting the comparative operation of narcotic poisons on the blood, or on the muscles and nerves, - i. 158-160
See also *Opium.*

Porter: galvanic theory of the causes of the superior briskness experienced in this liquor when drunk out of a pewter pot, - ii. 322

Premiums offered for galvanic researches, i. 89

Preparation of frogs, method of, i. 200, (note) ii. 303

Priestley, Dr. his experiments and theory on the galvanic pile, - - ii. 74,76-80

Q

Quicksilver; remarks on the employment of, in the production of galvanic phenomena, - i. 392
effects of a covering of mercury, on a metallic conductor, - - - i. 74
advantages of mercurial amalgams in the excitation of electricity in glass, - - ii. 231,232
its degrees of expansion different in different degrees of temperature, - - ii. 415
deflagration of quicksilver by the galvanic battery, ii. 395

R

VOL. PAGE

REINHOLD; his review of the different theories and opinions relative to galvanism, up to the commencement of the year 1798, i. 188
  diversity of hypotheses among different naturalists, i. 189, 190
  denominations employed, - - - i. 191
  early discoveries relative to the galvanic principle, i. 192
  plan of REINHOLD, in treating this subject, i. 197
  remarks respecting the choice and preparation of frogs, i. 199, 200
  contractions produced by animal substances brought into contact, - - - i. 201
    by non-animal heterogeneous substances, i. 202
    in dissected animals, by coating the muscles and the nerves, - - - i. 203
  application and effect of homogeneous coatings, i. 204, 205
  experiments with the ligature of the nerves, i. 206
  inquiry respecting the time during which the galvanic contractions can be excited, - i. 206, 207
  experiments respecting the influence of the galvanic fluid on those parts of animals not governed by the will; on the heart, - - i. 207
    the stomach, - - - i. 209
    the arteries, - - - i. 210-212
  contractions of the muscles by coatings of the nerves only, - - - i. 212, 213
    by coatings of the muscles only, - i. 213
    by coatings added to moist conductors contiguous to the animal, - - - i. 214, 215
  curious experiment on the vital force, and on the effect of the circumambient air, - i. 216
  experiments with various fluid conductors, i. 217, 218
  universal influence of the galvanic principle on animals and plants; causes why seldom perceptible in the latter, - - - i. 218, 219
  experiments relative to the external senses, i. 219
  practical experiments in different *media*, - i. 224
  conditions necessary to produce in animals the galvanic phenomena; a sensible fibre, - i. 226
    the vital force, - - i. 228
    incitability: (See that article) - i. 228, 229
    conditions essential in the arc, - i. 239
  constituent parts of the arc, and various observations respecting their power and modifications, i. 241

—general

VOL. PAGE
— general remarks on arcs, - - i. 250
considerations on the nature of the galvanic fluid, i. 252
definition and classification of the excitement, i. 253
inquiry whether the galvanic agent is properly entitled fluid, - - - i. 254
whether it is secreted in the animal, - i. 255
respecting the manner in which the galvanic phenomena are modified by the application of the arc, - - - - i. 256,257
conclusion respecting the nature of the galvanic fluid, i. 258
exposition of the hypotheses of different authors, on the galvanic fluid, - - i. 258
Respiration; the function of, a galvanic operation, ii. 402
observations on Dr. Crawford's theory of, ii. 403
on that of the French philosophers, - ii. 419
calculations of the quantity of air inhaled by a single inspiration, - - - ii. 421-424
respiration not the source of animal heat, ii. 427,429
changes experienced by the blood during respiration, ii. 430
question of the effect of respiration as a stimulus to the circulation of that fluid, - ii. 431,432
Review of the different theories and opinions relative to galvanism, up to the commencement of the year 1798, by Reinhold: (See his article), i. 188
Rheumatism, question of the efficacy of galvanism in cases of, - - i. 237,417,418,434

S

Sciatica, hypothesis of Galvani respecting the production of, - - - i. 399
various theories of; application of common electricity to its cure, - - - i. 458
application of galvanism to its cure, - i. 429
Scorbutic affections, experiments on the bodies of persons who had died from this cause, - ii. 188,189
application of galvanism to the cure of scrophulous swellings, - - ii. 457
Sealing-wax, galvanic properties of, - i. 57,58,72
Senses, experiments relative to. See particular articles.
remarks on the infinitude of sensations, *(note)* ii. 342
Sensibility; distinction of, from irritability, i. 181

VOL. PAGE
Shooting or *Falling stars*; production of, dependant on electricity, ii. 277
Sight, experiments respecting the influence of galvanism upon this sense, i. 149, 150, 220, 221, 293, 294, (*note*) ii. 314
application of galvanism as a remedy in cases of disorder of sight, i. 407, 417
its effect in ascertaining whether the operation for the cataract may be successfully performed, i. 419
in debility of sight, and gutta serena, i. 427
experiment, i. 452
(See also) i. 463, 465
mode of application of the battery, in this use, i. 431, 432
cases in the Author's experience, ii. 455
Silver; conductors of this metal recommended by Valli, as most powerful, i. 38
examples of contractions excited by the application of this metal alone, i. 388
Silver leaf; deflagration of, by the galvanic battery, ii. 134, 393
Smell, experiments respecting the influence of galvanism on this sense, i. 222, 296
Snow, remarks on the nature and effects of, ii. 275
Spasmodic affections; application of galvanism to the cure of, in the Author's experience, ii. 456-458
Spontaneous discharges from electric jars, theory of, ii. 265-268
Stomach; percussion on, more fatal than on any other part of the body, i. 176
experiments respecting the influence of the galvanic fluid upon, i. 209
Sue, M. his researches on the vital principle, i. 182
Sultzer; experiment by him connected with the principle of galvanism, prior to the discovery of that science, i. 1
not the foundation of galvanism, (*note*) i. 67
Surgery. See *Medicine*.
Susceptibility of animals, influence of different dispositions of the exciting arc upon, i. 375, 376
experiments and reflections on the means of varying, enfeebling, and re-establishing, in galvanic operations, i. 376-380
periods of the greatest susceptibility of animals, i. 383
Suspended animation, application of galvanism in cases of, ii. 463-469

T

VOL. PAGE

Taste, phenomena affecting this sense observed by Sultzer prior to the discovery of Galvani, i. 1, 2
 experiments by Volta respecting the influence of galvanism on the taste, - i. 68, 99
 his explanation of this phenomenon, i. 100
 a more singular experiment, - i. 127
 modifications of the taste of liquids drunk out of metallic vessels, explained, - i. 128
 effects of galvanism on the tongue compared by Fowler with those of electricity, - i. 148
 its effects on the cheeks and gums, - i. 161
 experiments by Humboldt on the tongue, which thus distinguishes metals, - - i. 283
 hypothesis of Fabroni, - i. 315, 316
 experiment producing at once the sensations of seeing and tasting, - - *(note)* ii. 314
 See also, i. 179-182, 220, 295, 343, 344, 350, 351, 354-357, ii. 324

Teeth, result of the experiment of Humboldt respecting the phenomenon of *setting the teeth on edge,* i. 278

Temperature. See *Heat.*

Temples, experiment of the application of galvanism to, i. 463

Tetanos, hypothesis of Galvani respecting the production of, - - - i. 400
 application of galvanism to the cure of, ii. 456

Teylerian battery, experiments with, - ii. 107

Theory, of Galvani, - - i. 12, 13
 conclusions of Humboldt respecting, i. 306
 Reinhold's review of the different theories up to the commencement of the year 1798: (See his article), i. 188
 Volta's theory, opposed and refuted, i. 306, 307, 381, ii. 5
 attempts of Aldini to reconcile it to that of Galvani, - - i. 390, 391
 his theory respecting the operation of the galvanic pile, - - ii. 9
 observations in opposition to, by Mr. Nicholson, ii. 22
 new theory of Volta on that subject, ii. 24
 report on, by a commission of the French National Institute, - - ii. 38

VOL. PAGE

— theory of HUMBOLDT, - i. 308-310
opposed by PFAFF, - - i. 327-340
theory of CREVE, - - i. 311
of LEHOT, - - - i. 346-349
of Dr. PRIESTLEY, respecting the phenomena of the Voltaic pile, - - ii. 76-80
theory of galvanic combinations, - ii. 320

Thunder and lightning, produced by electricity, ii. 276,277

Time during which galvanic contractions can be excited in animals, - i. 38, 39, 206, 207

Tin, advantages of this metal in the construction of vessels for certain galvanic experiments, i. 120
modifications of the taste of liquids drunk out of tin vessels, explained, - i. 122
deflagration of tin-foil by the galvanic battery, ii. 393

Torpedo, remarks and experiments relative to the electrical phenomena of, i. 25, 26, 317, 324, ii. 336
theory of VASSALLI-EANDI respecting, i. 320
conjecture of VOLTA respecting the nature of its electrical organ, - - ii. 36, 37
its structure, - (note) ii. 435
its analogy to the Voltaic pile, - ii. 8
nature of its shock, - (note) ii. 397
investigation of its electrical properties, ii. 398-400

Tortoises, experiments on, - i. 293

V

Vacuum; the idea of, incompatible with the explanation of the apparent exhaustion of a pneumatic receiver, - - (note) ii. 225

VALLI, Dr. experiments by, - i. 28-60

VAN MARUM, experiments by, - ii. 107-128

Vapour; experiments respecting the capacity of, to retain heat, - - ii. 411-415

Variation of the needle, - (note) ii. 280

VASSALLI-EANDI, his observations and experiments in the science of galvanism, - i. 79, 317, 449

Vegetables; irritability of the vegetable fibre, i. 166
influence of galvanism on plants, - i. 218, 288, 289
causes why it is seldom perceptible, - i. 218, 219

Velocity of the animal electricity may be increased without augmenting its degree, - i. 42
See also *Celerity*.

Vesicatories, experiments respecting the influence of galvanism on, - i. 284, 297, 298, 430

Vitality;

VOL. PAGE

Vitality; the electricity of animals independent of their vitality, - i. 41, 384, 385
- the electric fluid in certain cases the support of vitality, - - i. 43, 44
- the principle of life distinct from the nervous principle, - - i. 175-178
- the latter merely an appendant to, and shortener of, life, - - i. 178-181
- researches of M. SUE on the vital principle, i. 182
- curious experiment by REINHOLD on the vital force, and the effect of the circumambient air, i. 216
- the vital force in an animal essential to the production of galvanic phenomena, - i. 228
- experiments by TOURDES respecting the vitality of the blood, - - - i. 64
- by VALLI, to determine the time during which animals can live without nourishment, i. 54
- experiments to revive animals killed by drowning or by asphyxia: (See also *Asphyxy*), i. 49-51
- on the revival of birds, - i. 299, 300

VOLTA; his experiments, and observations, i. 16, 17, 92-95, 99
- his theories. See *Theory*.

Voltaic pile, and *battery*. See *Pile*, and *Battery*.

W

Water, its effect (hot, or cold) in animal electricity, i. 57
- its use as a conductor of animal electricity, i. 63
- its effect in this character, on the movement of the galvanic fluid in the interior of the pile, ii. 164-170
- influence of electricity in the process of the crystallization of water; and on water in the state of ice, - - - i. 216, 217
- in the state of vapour, - i. 217
- different capacities of ice and water to contain the electric fluid, - - i. 222, 223
- principle of the diffusion of air in water, i. 224
- beneficial effects of the changes into ice and vapour, - - i. 274
- its proportionate conducting powers, to those of metallic bodies, - - i. 362
- its degrees of expansion by heat at different temperatures, - - - i. 415, 416
- its resolution into oxygen and hydrogen gas by the galvanic fluid, - - i. 135

VOL. PAGE

— Decomposition of water; observations of Mr. Henry relative to the performance of this process by the galvanic pile, - ii. 68
Dr. Priestley's opinion respecting, ii. 75
experiments of Dr. Wollaston, - ii. 91
process of, on a new plan, - - ii. 127
theory of the French philosophers respecting, ii. 151
process of Mr. Nicholson, - ii. 376
of Richter, and others, - ii. 377,378
of the Author, - - ii. 379
its decomposition by electricity, - ii. 380
elucidated in the galvanic operation, ii. 382
detail of the process of decomposition, ii. 384
Dr. Gibbes's theory of the component parts of water, - - ii. 385
experiment respecting the operation of the two wires employed, - - ii. 386
hypothesis on this subject; illustrated, ii. 387,388
Water-spouts, phenomena of, - ii. 285-289
Will; experiments respecting the influence of the galvanic fluid on those parts of animals which are not governed by this faculty, i. 207-212, 266, 302-305, ii. 197
Wing of a fowl, experiment on, - i. 60
Wires, metallic; employment of, in the decomposition of water, - - ii. 376-389
See also *Deflagration*.
Wood; its different capacity, as a conductor of electricity or otherwise, in a moist and in a dry state, ii. 217
in a state of charcoal: (See *Charcoal)*, ii. 217
properties of different sorts of wood as materials for charcoal, - - ii. 331
Wollaston, Dr. his experiments and inquiries with the Voltaic pile, - - ii. 86
Worms, experiments on; earth worms, i. 145,146
aquatic worms, - - i. 290

Z

Zinc; singular phenomenon respecting a bar of, observed by Humboldt in an experiment, - i. 273,274

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Printed by B. M'Millan,
Bow-Street, Covent-Garden.

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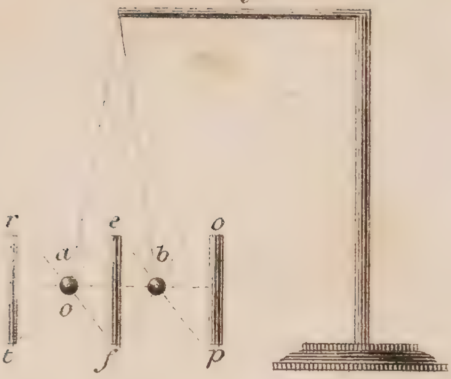

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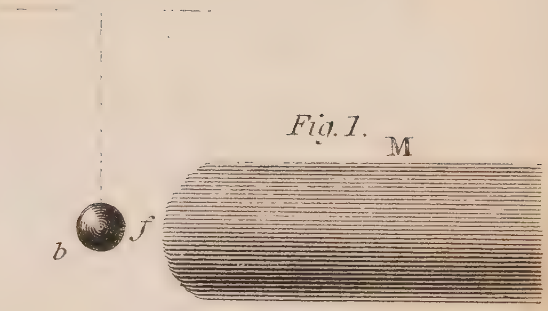

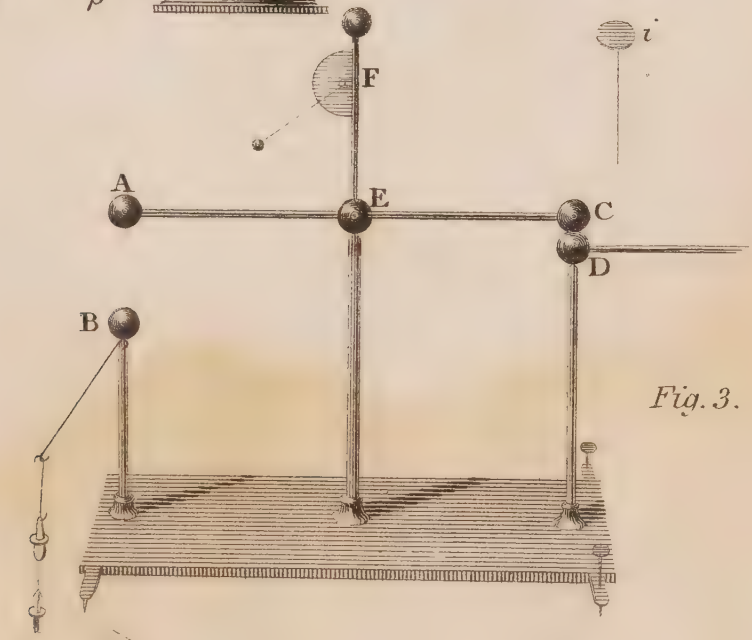

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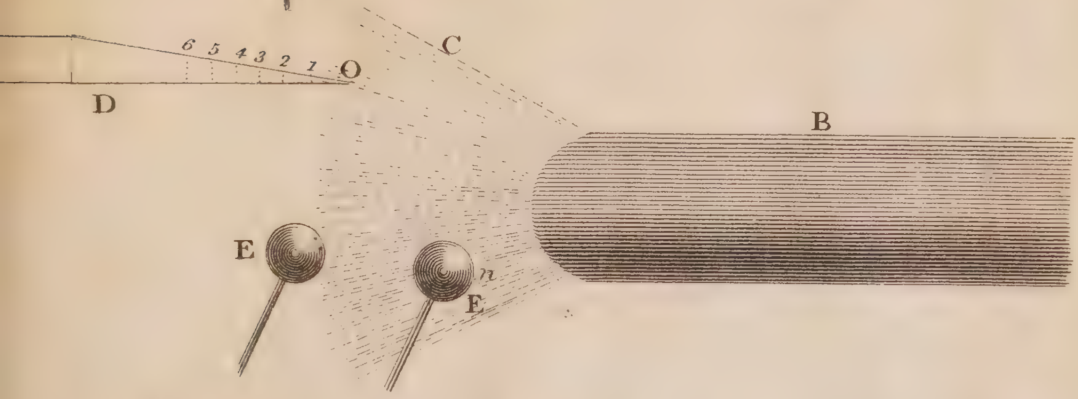

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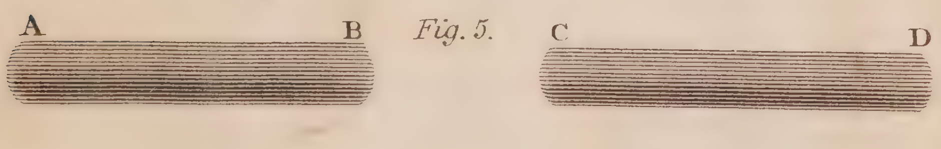

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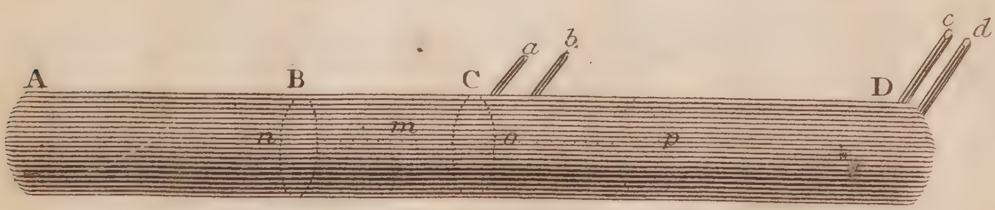

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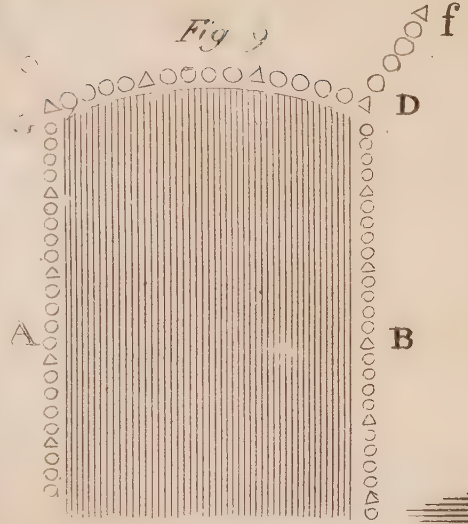

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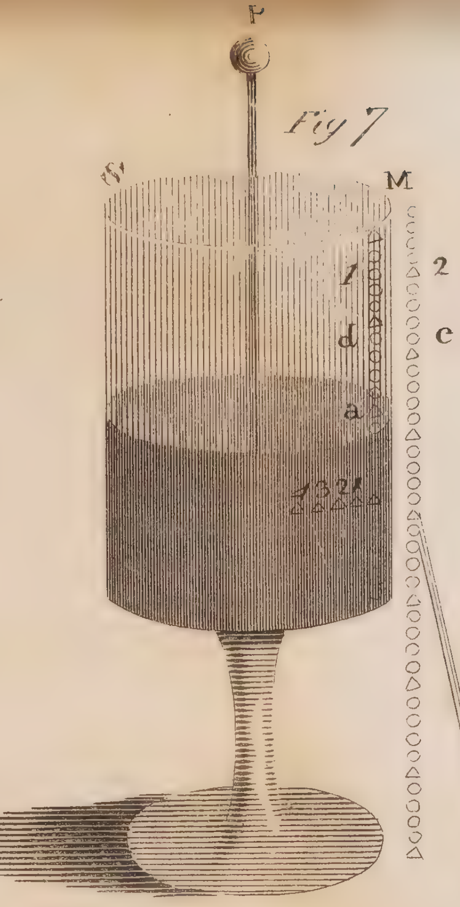

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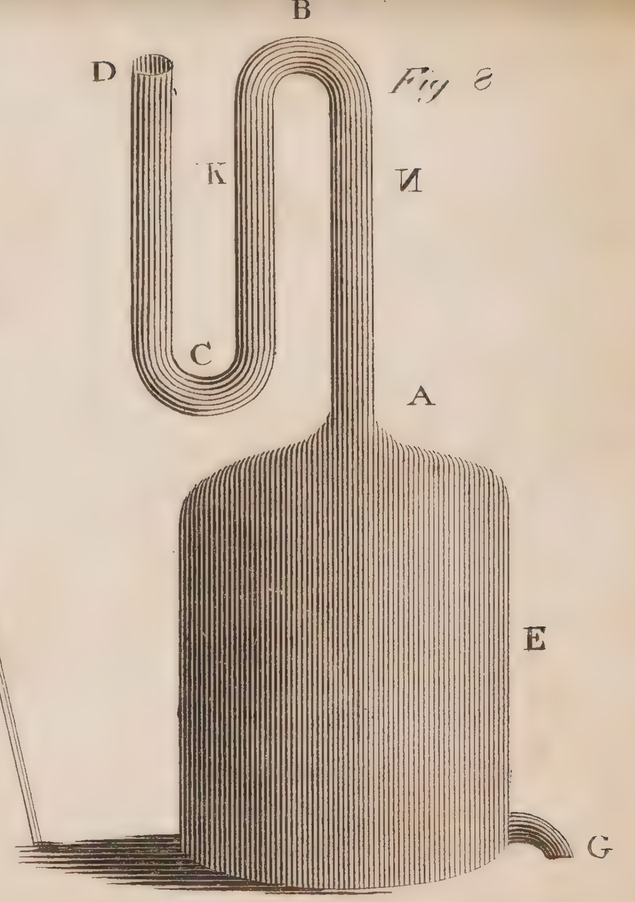

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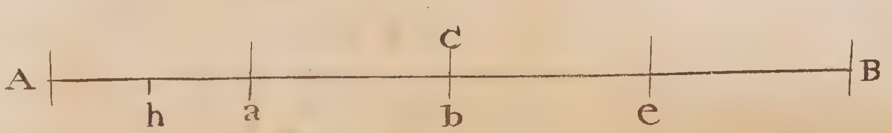

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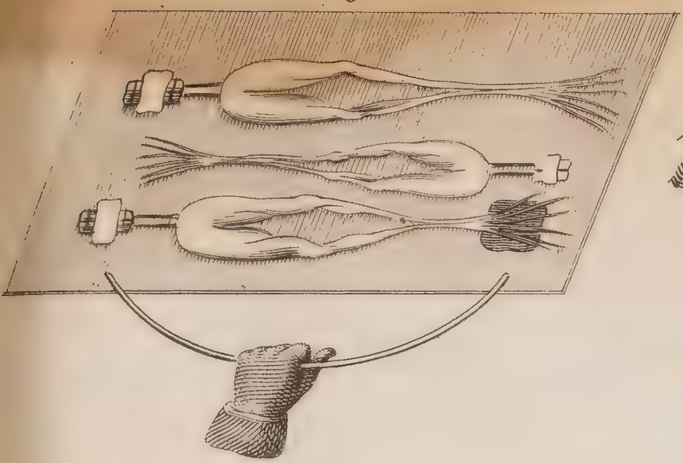

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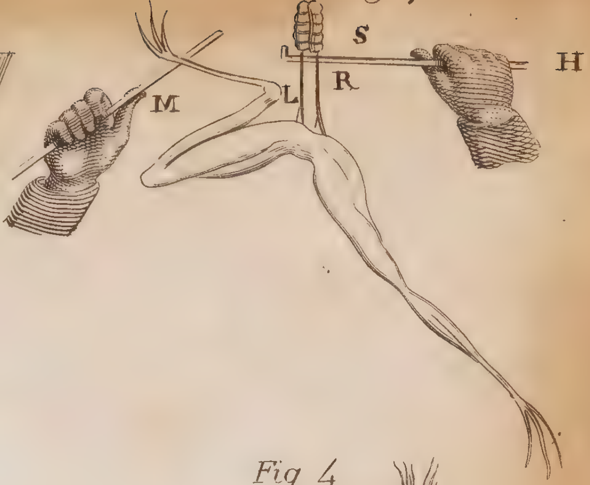

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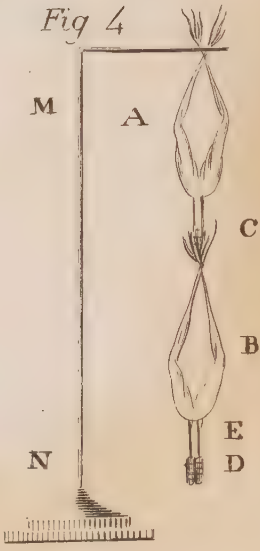

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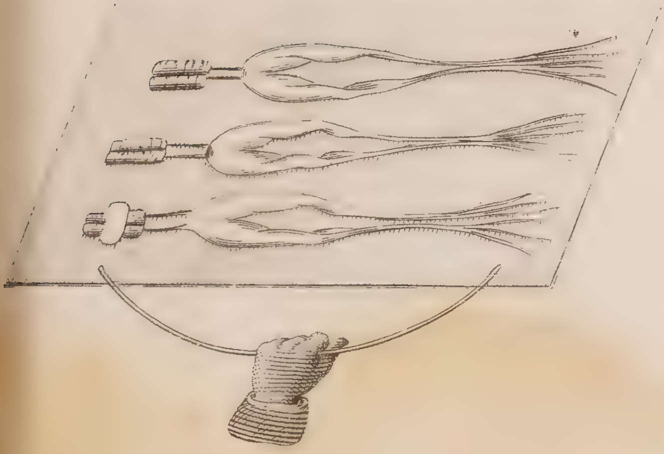

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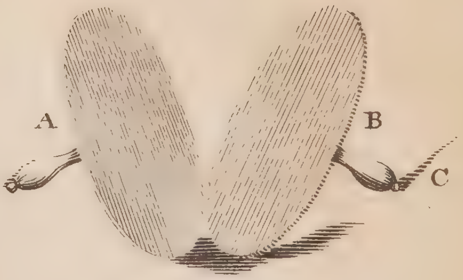

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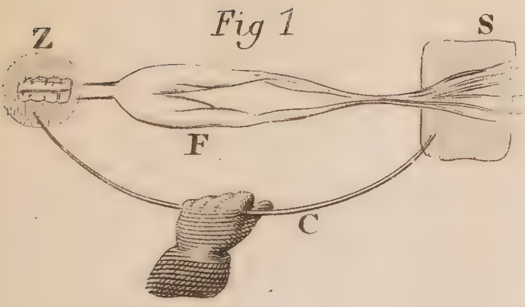

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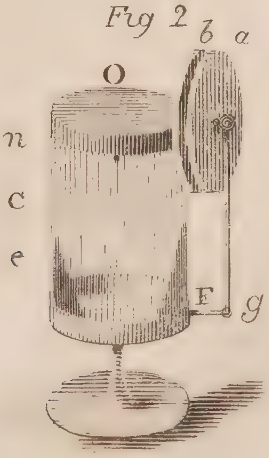

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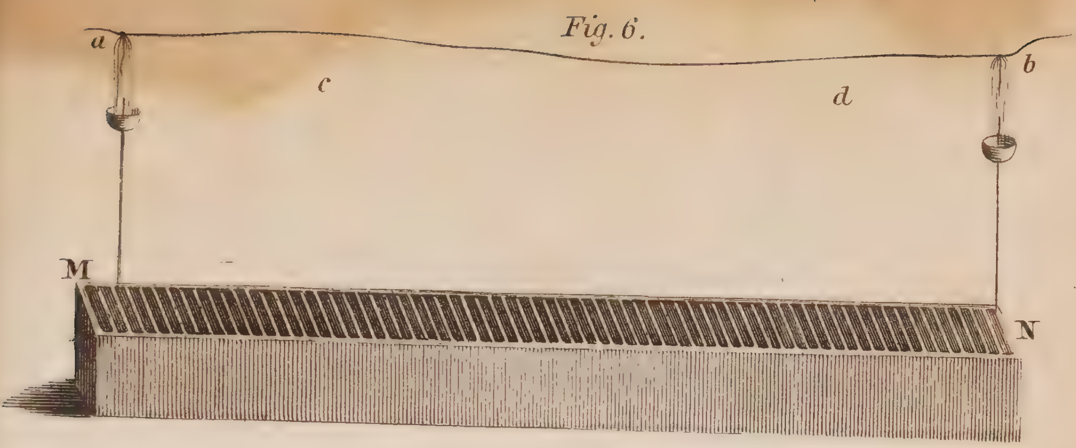

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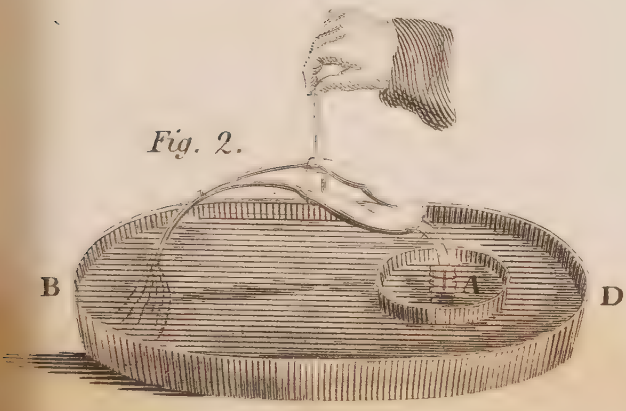

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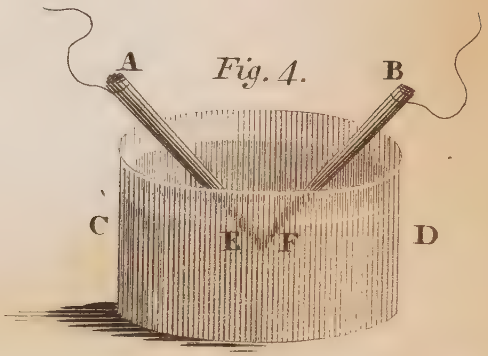


Fig. 3.

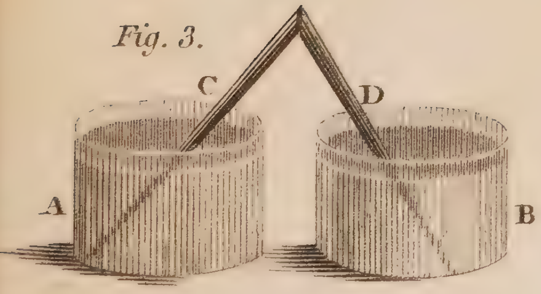
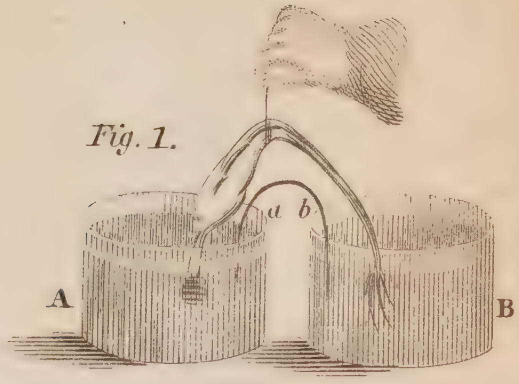


Fig. 1.



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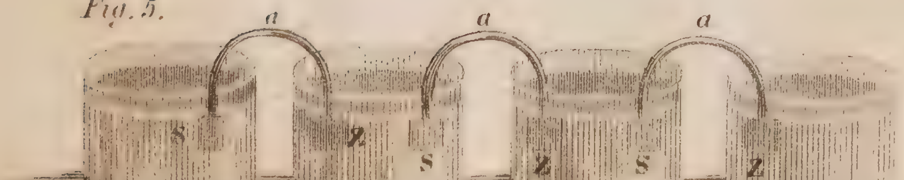
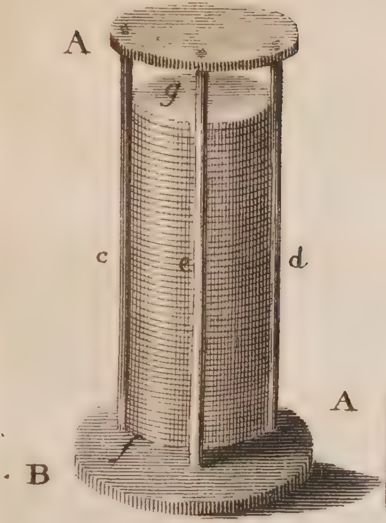


Fig. 1.



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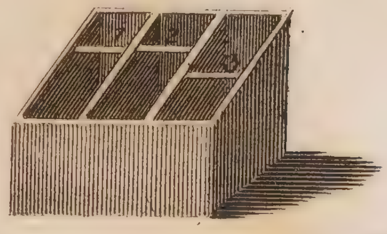


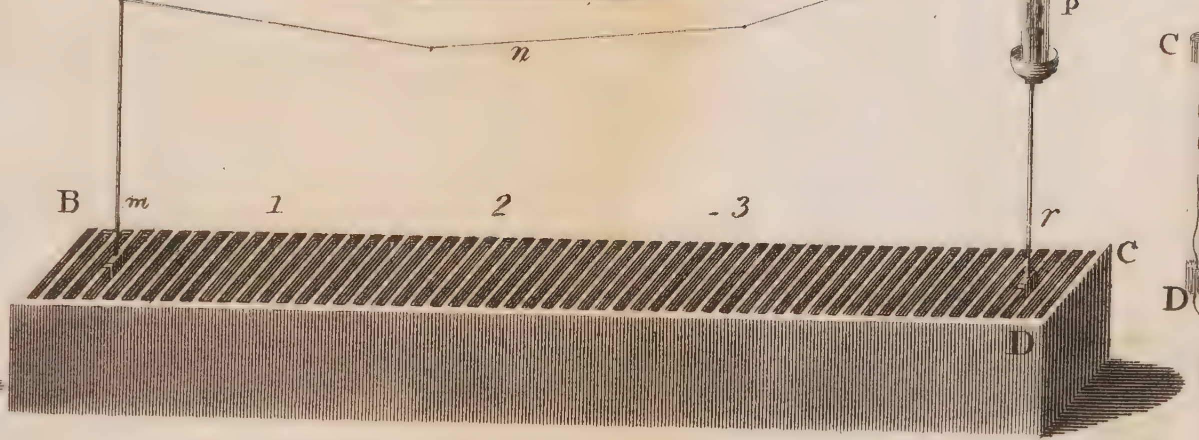
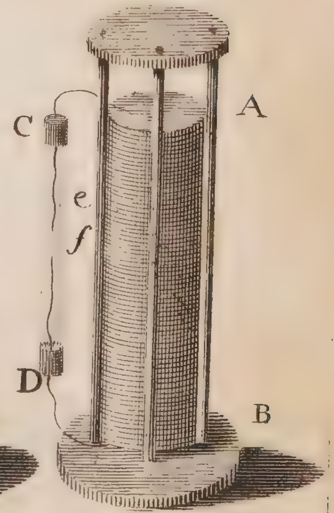


Fig. 2.

Fig. 3.



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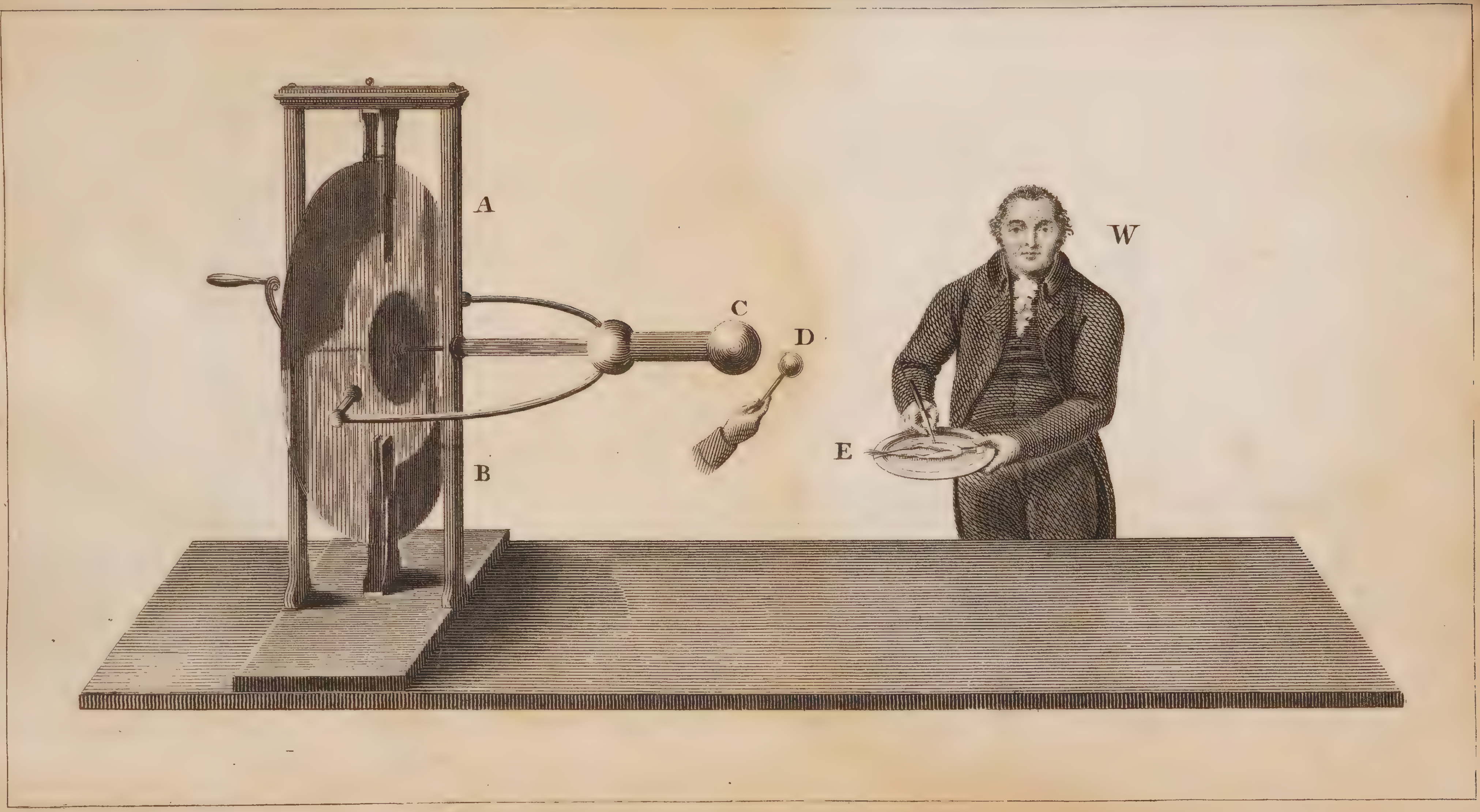

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Pl. II

Fig. 1

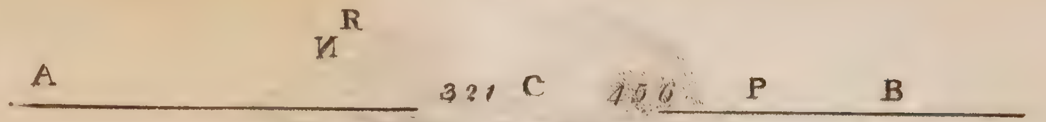
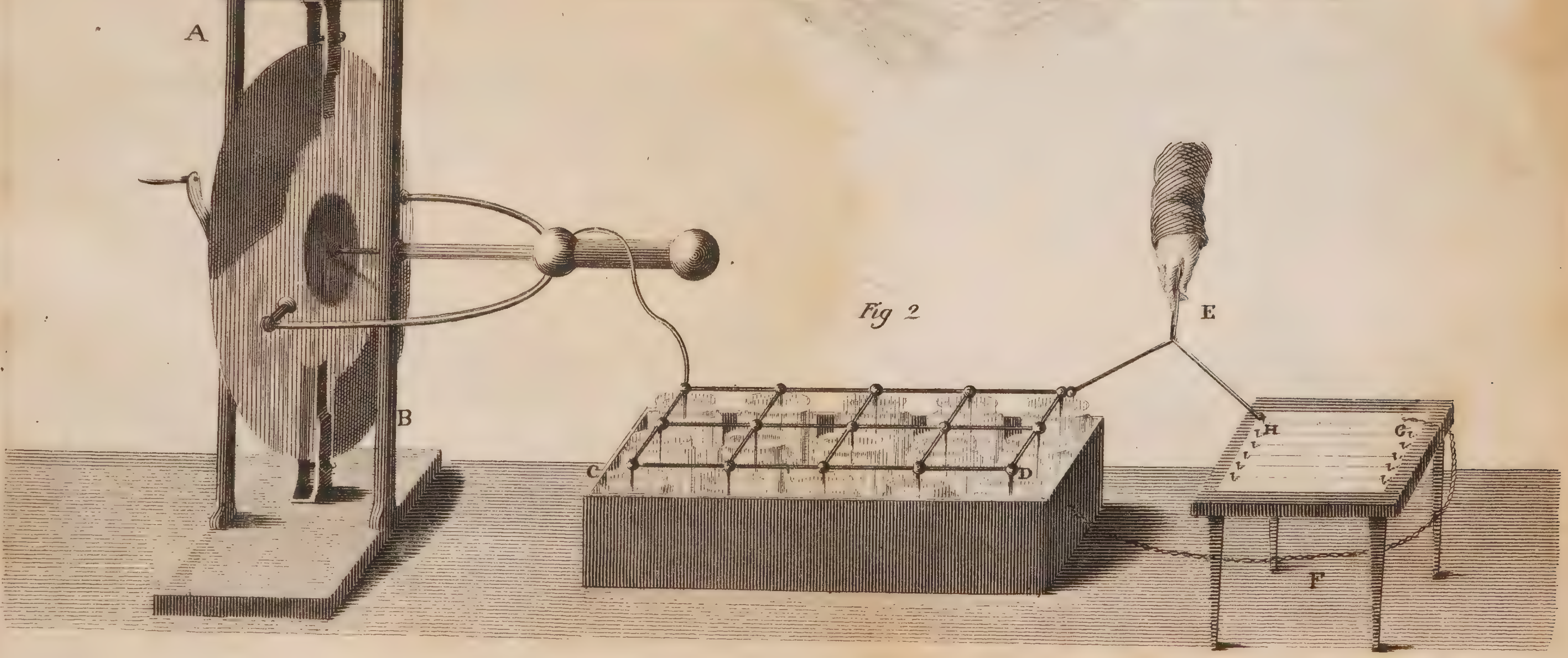


Fig 2



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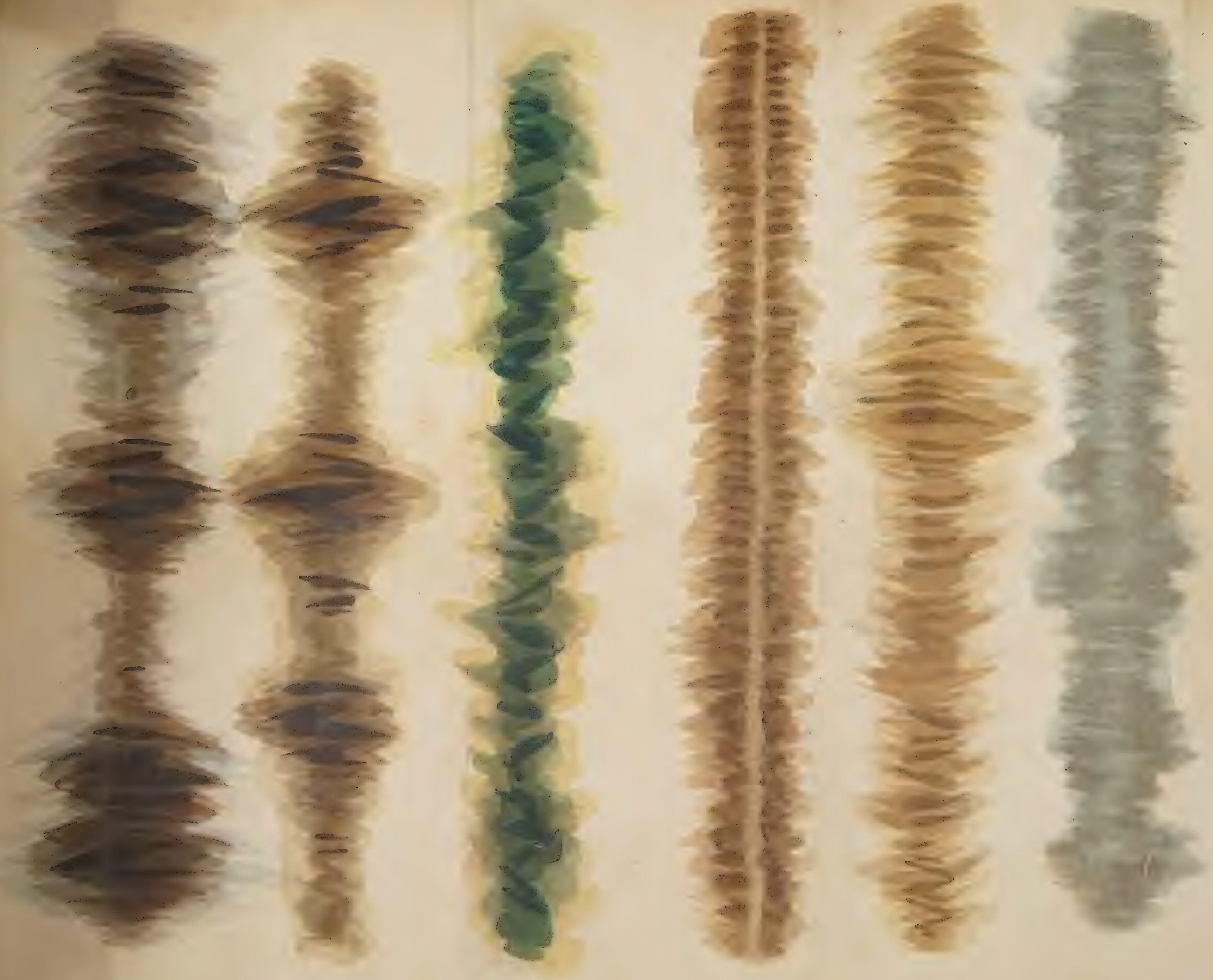


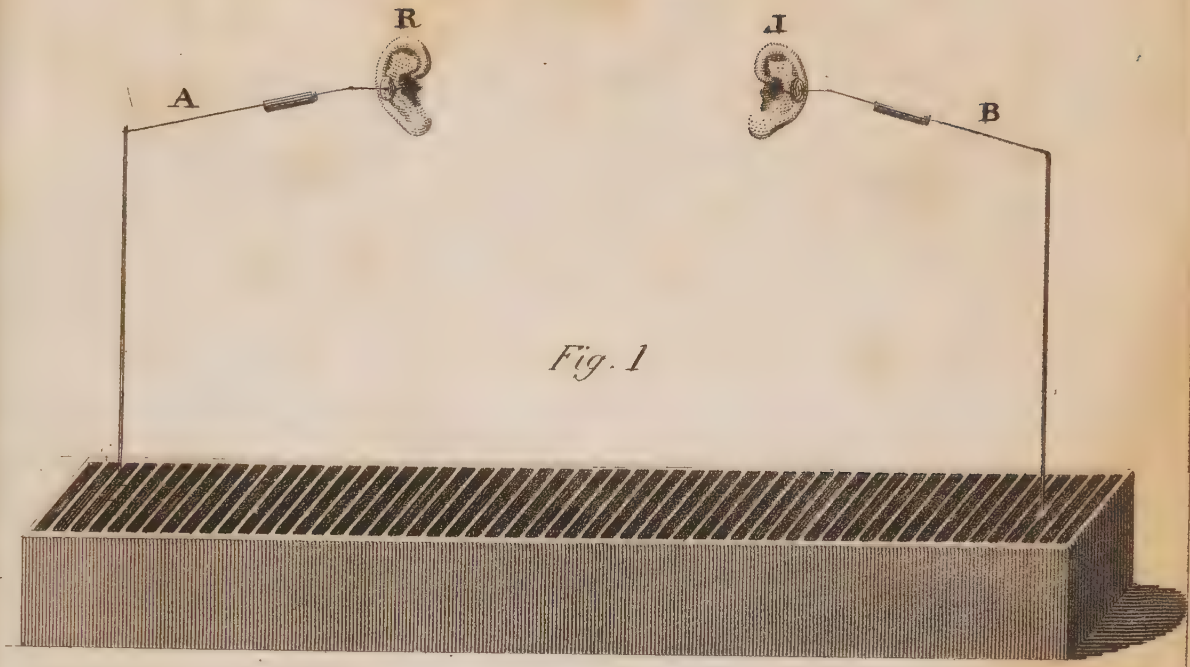


Fig. 1

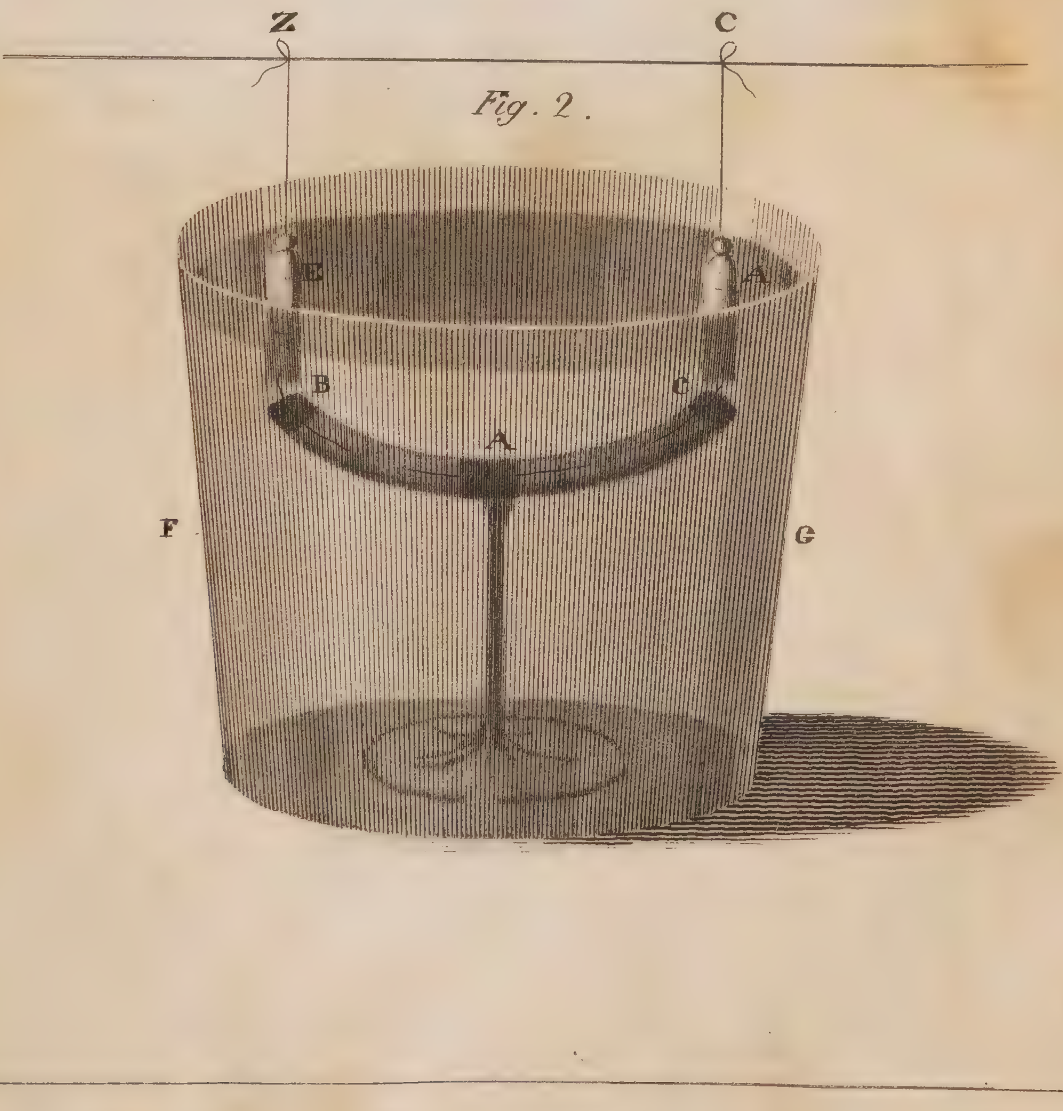
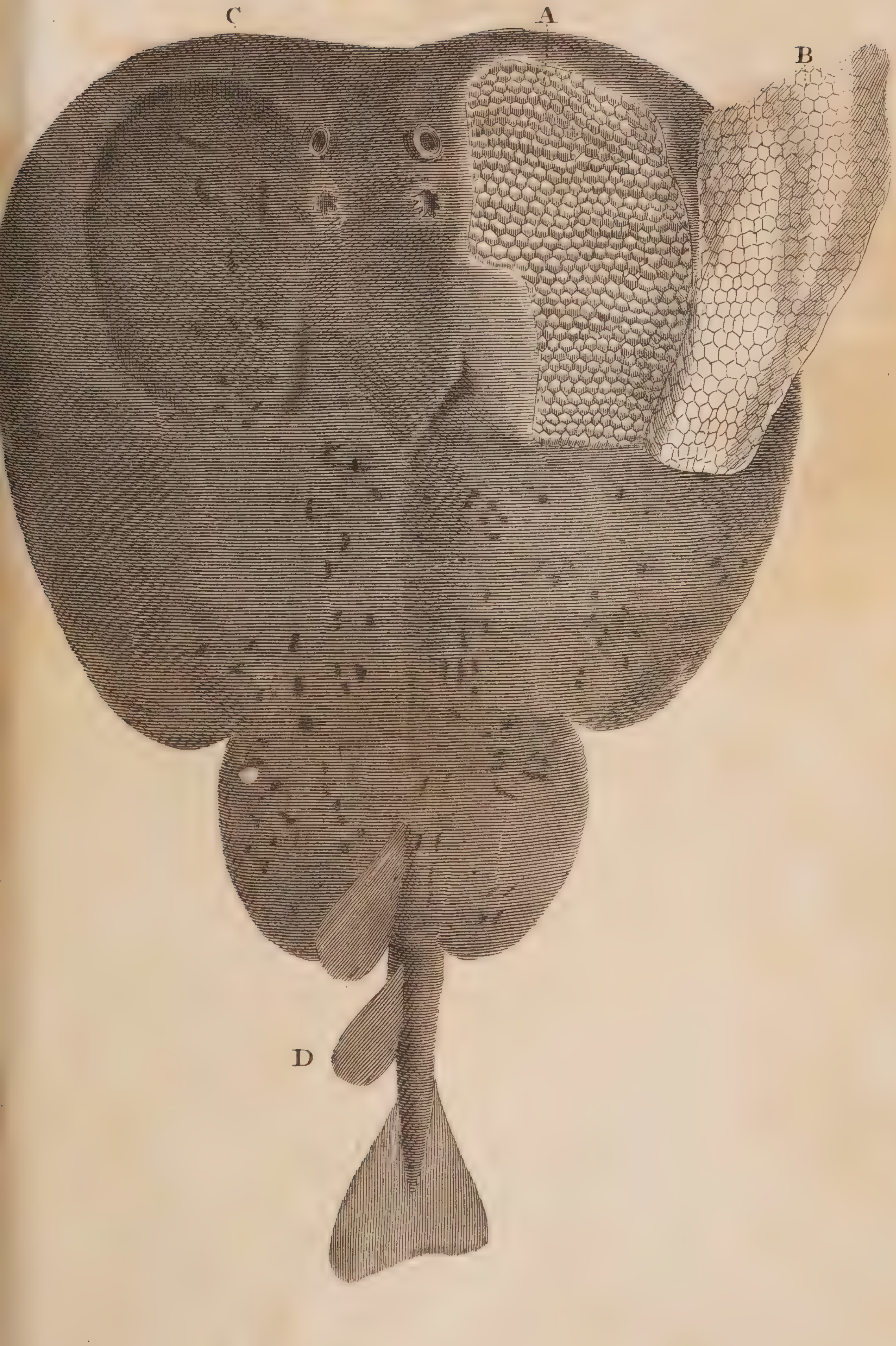


Fig. 2.


C
A
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